Banking on Jobs: Welfare and Credit

Mayuri Chaturvedi*   Asad Rauf†

Abstract

This paper explores how a job guarantee welfare program interacts with credit markets. We analyze the impact of a country-wide job guarantee program, launched in India in 2006. Exploiting the staggered implementation of the program we estimate the treatment effect of the program on household and commercial borrowing. We find that the program led to an increase of up to 23 per cent in credit outstanding in districts which received the treatment first. Personal borrowing increased by up to 27 per cent and commercial borrowing by around 18 to 20 per cent. We do not find conclusive evidence of any effect on later treated districts. We rationalize our findings as relaxation of credit supply constraints because the program reduced information asymmetries between banks and rural households. Our findings shed light on the complementary relation between credit markets and welfare states.

JEL-Classification: D14, D15, D25, G21, J20
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*University of Liverpool, Liverpool, UK; email: mayuri.chaturvedi@liverpool.ac.uk
†University of Groningen, Faculty of Economics and Business, Groningen, Netherlands; email: a.rauf@rug.nl.

1 Introduction

Do welfare states complement or substitute credit markets? Traditional economic theory advocates for government intervention only in the presence of market failures. Low-income households often borrow to maintain living standards when redistributive mechanisms are limited (Kumhof, Rancière, and Winant, 2015; Rajan, 2010; Montgomerie, 2013; Ahlquist and Ansell, 2017). In the presence of social security, borrowing to smoothen consumption falls (Bell and Mukhopadhyay, 2020). However, there are at least two ways in which stronger welfare states could complement financial markets and result in increased borrowing. First, a growing literature shows that investments in state capacity (to improve tax compliance and reduce leakages) supports market development (Besley and Persson, 2011; Besley, Ilzetzki, and Persson, 2013; Besley and Persson, 2010). Second, the political economy of credit highlights the complementary nature of strong welfare states and permissive credit regimes. Public welfare provides support to vulnerable households, with middle and upper-income households getting liberal access to formal credit (Wiedemann, 2021).

In this paper, we study the impact of a job guarantee program on formal credit markets. We focus on the *Mahatma Gandhi National Rural Employment Guarantee Scheme* (NREGS), the largest job guarantee program in the world.[1] We find significant increase in bank credit in districts that received the program first, with the result mainly supported by personal and industrial credit. We also find a substantial increase in bank accounts in early NREGS districts. We argue that efficient welfare delivery necessitates improvements in state capacity that potentially enhance the efficiency of financial markets. This subsequently deepens the reach and accessibility of formal credit institutions for all.

Although economic theory sheds considerable light on the effect of such programs on labour markets (Besley and Kanbur, 1991; Besley and Coate, 1992; Basu, Chau, and Kanbur, 2009), the literature provides little guidance on the impact on credit markets. Indeed, several studies have documented evidence that NREGS impacted the private labour market and wages (Imbert and Papp, 2015; Zimmermann, 2023; Muralidharan, Niehaus, and Sukhtankar, 2023). Some studies have also looked into the welfare consequences of household consumption (Ravi and Engler, 2015; Berg et al., 2018; Deininger and Liu, 2019), environmental impact (Behrer, 2021), and aggregate impact of the program on local economies (Cook and Shah, 2022). Our paper contributes to this literature by highlighting the effects on household and firm borrowing, and the potential mechanisms.

The NREGS program has several features that allow us to explore the effects of such a 'workfare' policy using non-random, observational data. The program was introduced by the Indian government in 2006 based on the *Mahatma Gandhi National Rural Employment Guarantee Act 2005*, which provides every *rural* household the right to demand up to 100 days of manual labor jobs per year for minimum wages. First, the program was introduced in three phases in a staggered manner across the 625 districts of the country. The phased implementation aids in the empirical identification strategy. Second, the at-scale implementation of

[1] In the first 8 years of the program, NREGS generated over 100 million person-days of work.

the program enables a study using aggregated data without external validity concerns. Third, districts in India are "mini-economies" by themselves, enabling us to explore general equilibrium effects, for example, the program's impact on industrial and trade credit.[2] All these taken together provide a conducive setting to implement a quasi-experimental research design using district-level data.

The phased implementation of the program was as follows. *Phase-1* districts received the program in 2006, *Phase-2* districts in 2007, and the remaining *Phase-3* districts in 2008. We utilize this staggered adoption to estimate the effect of the program on household and firm borrowing using a difference-in-differences approach (similar to Imbert and Papp (2015), Cook and Shah (2022)). Using India's central bank's detailed data on district and category-wise loans, made by all commercial banks in the country, we are able to disentangle the effect of the program on different kinds of credit (e.g. personal loans, industrial loans, etc.).[3] This allows us to provide insights on what the underlying mechanisms could potentially be.

We find that districts that received the program earlier (Phase-1 districts) saw an *increase* in credit outstanding of around 21%. We do not find evidence of significant impact on credit in districts that received the program later (Phase-2 districts).[4] The main feature that differentiates Phase-1 districts from others is that they are poorer districts with predominantly rural and "informal" economies. This naturally raises identification concerns which we address later. The key point here is that a job guarantee welfare program would not always have an impact on the credit volumes. There are features of the program, and characteristics of the recipient districts, which play a role. We explore these further.

One-fourth of total credit outstanding in Phase-1 districts were personal loans (i.e., loans to individuals and households). The increase in total credit in Phase-1 districts were driven by higher personal borrowing which increased by upto 27%. These findings are counter to much of the prior work which sees the relationship between financial markets and welfare states as substitutive (Rajan, 2010; Kumhof, Rancière, and Winant, 2015; Ahlquist and Ansell, 2017), whereby we would expect borrowings of low-income households to come down. Households borrow to maintain living standards when welfare policies are limited. Thus a large, redistributive, anti-poverty program should reduce borrowing. An increase in borrowing, however, hints at mechanisms that are not well understood. Under certain regimes, the structure of welfare states could complement credit markets. For instance, when welfare policies are not comprehensive and credit markets not deep (both arguably true in India), welfare programs can potentially help poor households access credit (Besley and Persson, 2011; Besley, Ilzetzki, and Persson, 2013; Wiedemann, 2021).

There were two key features of the NREGS which help us understand the observed impact on personal loans. All participants were required to have state identification cards ('job cards') and wages were to be deposited only in the bank accounts of program beneficiaries (Sukhtankar, 2016).[5] Due to this policy

[2]The median district is similar to the population of Philadelphia.

[3]We use the central bank's (Reserve Bank of India) *Basic Statistical Returns* district-level data set. The data set contains information on the universe of *all* scheduled commercial banks (SCBs) in India.

[4]We are not able to estimate any treatment effects for the last treated (Phase-3) districts because no control group remains after their treatment.

[5]Initially, some states were allowed to pay wages using cash if there were significant infrastructure and banking system limitations. But the program soon shifted to only bank transfers as the mode of wage payment.

requirement, NREGS helped bring a large population – mostly unskilled labor – under the umbrella of the banking system. The availability of bank accounts with proper IDs was a large fixed cost needed for banks to be able to cross-sell related banking services to these participants. Without a valid state ID, it is very costly for banks to verify the identity of the customer, the address for collection, communication details, and most importantly, borrowing history. We find evidence NREGS caused a significant increase in rural bank accounts (increase of around 9%) and brought many households within the banking ambit. We argue that this specific feature of the program reduced information asymmetries between banks and low-income households and as a result relaxed credit supply constraints. We provide a formal exposition of this argument by modifying the Stiglitz and Weiss (1981) model. Since Phase-1 districts were the more "backward" districts, the scope for banks to expand their networks in these districts was larger. With the government bearing much of the costs to get beneficiary households to create bank accounts, the banks plausibly found it profitable to expand their lending in these areas.

The findings also provide insights on investments in state capacity supporting market institutions as discussed in Besley and Persson (2011). Our findings suggest that when a welfare scheme is designed such that it provides a solid foundation to the financial markets in a country, then welfare schemes can indeed complement credit markets by increasing participants' access to credit.

We next turn our attention towards commercial borrowing. We find that industrial loans (i.e., borrowings by manufacturing firms) increased by 20% and trade services loans (i.e., borrowings by wholesale and retail firms) increase over 17%. These two categories comprise more than 1/3 rd of total loans outstanding in Phase-1 districts. One explanation of these findings is related to the mechanism of increased consumption from households. The ripple effect of more stable incomes could have boosted aggregate demand for firms and non-participants[6]. The increased demand led firms to borrow more for investment in increasing production capacity. However, evidence from firm data does not corroborate such a hypothesis. Firms in treated districts did not report increased sales (Agarwal et al., 2021). An alternative, plausible explanation is labour-substituting capital investment by firms due to NREGS. Since NREGS increased private sector wages (Imbert and Papp, 2015; Muralidharan, Niehaus, and Sukhtankar, 2023), firms faced increased costs of production. In response, they moved towards less labour-intensive technologies by borrowing from banks to make such investments. Agarwal et al. (2021) find evidence that after NREGS, firms faced an adverse labour-supply shock and resorted to increased mechanisation. Our findings of increased industrial loans sit consistent with their evidence of increased firm investments and mechanisation.

The findings on the increase in commercial credit are important in the broader context of welfare programs. Besley and Coate (1992) explore the trade-offs of publicly funded employment guarantee. They argue that the benefits from poverty alleviation must exceed the costs due to lower private-sector earnings. Our empirical evidence suggests that the general equilibrium effects on firm dynamics can be substantive. When the policy is large enough to move private sector wages, firms respond by levering up. Thus, negative

[6]Any direct benefit transfer (DBT) schemes will also have a similar impact on the banking system. NREGS was one of the first and largest government expenditure programs in India (with an expenditure of Rs 1,11,500 crore or USD 15 trillion in FY 2020-21), and is likely to have had a bigger impact than the other DBT schemes

consequences of high firm debt (e.g, expected financial distress costs, agency costs, etc) could be a potential source of costs that policy makers need to bear in mind when evaluating such policies.

The identifying assumptions used to estimate these effects merit some discussion. The districts were not assigned randomly to the three phases. On the contrary, the program was rolled out in the more 'backward' districts first, and later in the relatively well-off districts. Therefore, to assume that outcome variables would trend parallelly, in absence of treatment, would be hazardous. The 'formula' to assign districts to phases, however, was distinct. The government used a two-stage assignment algorithm to allocate districts to implementation phases: In the first step, each Indian state received a quota of treatment districts proportional to the percentage of India's poor living in that state. In the second step, the quota in each state was filled with the poorest districts according to a development ranking (Zimmermann, 2023). The government published a white paper which provided the district wise values of the socio-economic metrics used in this algorithm (Planning Commission, 2003). We argue that when conditioning on those exact pre-treatment metrics, the assignment of districts to phases is independent of outcome variables of interest (in our case borrowing activity in those districts). We provide evidence in support of this argument.

Our paper is closely related to Cook and Shah (2022). They find NREGS led to overall increase in aggregate economic output by 1-2% per capita, measured by night-time lights. Our paper focuses on the dynamics of the credit markets. We explore how and why a public works program will impact the market for credit. Another paper closely related to ours is Bell and Mukhopadhyay (2020). They find that in lean agricultural seasons, borrowing by rural poor households reduces by the amount of the NREGS workfare wage. On the other hand, during cultivation season, workfare wages lead to an increase in borrowing, especially by landed households. Our paper differs from their paper in two important dimensions. First, their analysis is limited to a sample of agricultural rural households in a couple of districts in one state (Odisha) and focuses on informal sources of lending (e.g., by local moneylenders). Since the program was implemented at the state level, implementation efficiency varied widely by state. Results true for one region may not apply generally to other regions. Our paper addresses this issue by looking at the *universe* of all lending by scheduled commercial banks across India. Second, we provide a novel mechanism for the increase in formal bank credit in NREGS districts, namely improved state efficiency enhancing the efficiency of formal financial institutions, thereby easing their credit supply. Muralidharan, Niehaus, and Sukhtankar (2016) discuss mechanisms and provide evidence for improvements in state capacity driven by the NREGS program. We extend the link between state capacity and efficient delivery of public programs to financial market efficiency.

The rest of the paper is organized as follows. Section (2) provides some institutional details related to the program execution and the Indian banking sector to contextualize the results. Section (3) lists the data sources and explains the empirical methodology and the identifying assumptions. Section (4) discusses the results. Several robustness checks in Section (5). Section (6) presents a model to formally describe the mechanism. Finally, Section (7) concludes.

2 Institutional Context

The Mahatma Gandhi National Rural Employment Guarantee Scheme (NREGS) was introduced in the year 2005 to "enhance livelihood and security in rural areas by providing at least 100 days of wage employment in a financial year to every household whose adult members volunteer to do unskilled manual work". The scheme was first introduced in 200 districts in year 2005-06, 130 districts in year 2006-07, and another 295 districts in 2007-08. The districts were chosen based on a development ranking on three indicators; *Total Scheduled Caste & Tribe Population percentage*, *Agricultural Wages*, and *Agricultural Productivity per Worker*. Districts that scored the lowest on these parameters received the program first. There was an additional federal equity consideration that all states receive the program in at least one district in year one. We refer to the first group of districts as *Phase 1* districts in the subsequent analysis.

The program was only available in rural parts of districts, with part of the program funded by the central government and the remaining funds coming from state governments. About 60% of the program outlays are reserved for payments to unskilled labor, 34% for purchase of materials, and the remaining on covering administrative costs. The insistence on direct beneficiary transfers (i.e., paying program beneficiaries directly in their bank accounts) was primarily to reduce leakages and corruption. There is considerable variation in the way the program is implemented across the twenty-nine states and seven union territories in India (Sukhtankar, 2016). The daily wages are set by each state at a level similar or slightly lower than the state's statutory minimum wage. As such the program is said to be 'self-selecting' in that the most needy and poor participants without any alternative employment opportunity choose to do the tedious manual labor required on the NREGS job site.

Households are required to get a jobcard from the local government office which contains the names of all adult members in the household. It also serves as a record of the work done and payments received. Once they have the jobcard, households can apply for work whenever they need it. The local government is responsible for providing employment within 15 days and within 5 kilometers of the applicant's home. If employment is not provided within 15 days, the applicant is entitled to unemployment allowance and if the work is farther than 5 kilometers, they will get a travel and subsistence allowance. Payments for the work done are supposed to be made weekly and no later than two weeks after the work is done. Initially, payments were made in cash by the same administrative bodies but the recent guidelines call for payments to be made through bank or post office accounts and for separation between the officials who implement and pay.

NREGS may have transformed the banking environment in rural areas in multiple ways. First, the program works by directly depositing wages in bank accounts of beneficiaries. A state ID proof such as a NREGS job card or *Aadhaar* card (unique ID for every Indian resident) is needed to register for the program and avail work. The combination of mandatory state ID along with wages deposited directly in bank accounts presumably could have lead to reduced entry barriers into formal banking for rural participants. Second, there is (suggestive) evidence that NREGS may have increased the viability of the business correspondent (BC) model in remote rural locations (Kochar, 2018). In India banks use local agents, known as business correspondents (BCs), and new mobile technologies to enable branchless banking. Since the program put money in the bank accounts of beneficiaries, in the absence of a bank branch they needed BCs to access to their accounts for withdrawals. This may have led to an increase in demand for BC services in unbanked rural locations. Since the fee per transaction is very low, BCs rely on volume of transactions for sustaining their incomes. Without the demand from NREGS households, it would not make business sense to provide the service in that area. In this manner, NREGS potentially increased the access to banking services for non-NREGS households also (Desai, Vashishtha, and Joshi, 2015).

3 Data & Empirical Methodology

3.1 Data

We collect data from the *Basic Statistical Returns (BSR) of Scheduled Commercial Banks in India* series provided publicly by the Reserve Bank of India. The dataset provides information on various characteristics of bank credit, based on data submitted by commercial banks. The dataset contains district-wise information on credit outstanding, based on type occupation/activity and category of the borrower. Importantly, the BSR data encompasses the entire population of loans provided by commercial banks in India and is, therefore, representative of the commercial lending activity in any given district.

The BSR data provides a snapshot of the stock of loans and deposits in each district as of the 31st March of every year.[7] The data further classifies the total stock of loans into occupational categories such as: *Agricultural*, *Personal*, *Industrial*, etc. In the appendix we define each category of these loans and what constitutes them. Our sample runs from 2002 to 2008. We end the sample in 2008 because all districts in India had received the NREGS program by 2008 and, hence, there is no control group left for comparison.

The NREGS implementation schedule across districts was collected from the their website. As mentioned earlier, the implementation schedule of the program across districts was not random. There was a 2-step assignment procedure. In the first step, each Indian state received a quota of treatment districts proportional to the percentage of India's poor living in that state. In the second step, the quota in each state was filled with the poorest districts according to a development ranking. We observe the variables, based on which the 2 step assignment was executed, for 447 districts of India. This data was obtained from a detailed study that was completed by the Planning Commission of India in 2003 to identify districts in the most populous states of the country where the NREGS program would be rolled out first. We limit our analysis to these 447 districts, such that we can use a weaker assumption of *conditional* common trends.

One of the major challenges with doing a panel data analysis at the district level in India is that the changes in variables across time are induced mechanically because of district delimitations. District delimitation is a very common phenomenon in India. For example, in 2001 there were 593 districts in India. This increased to 748 by end of 2021. To overcome this challenge, we focus on district boundaries as of 2001. We focus on 2001 because that was the most recent year (compared to our sample period) where there was

[7] The financial year in India runs from 1st of April to 31st of March.

a countrywide population census. More specifically, if any district was divided into multiple districts between 2002 and 2008, we sum the observed values of the outcome variables to represent the (hypothetical) undivided district.

Some new districts were carved out of multiple districts. These districts pose a real challenge in identification because these cannot be combined with their erstwhile parent districts. The yearly change in the stock of loans would therefore be sullied by the delimitation process and would not represent changes brought about by economic factors. We drop such districts and their erstwhile parents from our analysis. After combining and/or dropping delimited districts we are left with a sample size of 420 districts.

[Insert Table 1 here]

In Table 1, we report the summary statistics of our main variables of interest. We note that agricultural, industrial, and personal loans form the bulk of loans outstanding, together constituting more than 75% of the loans. Phase-1 districts have the lowest amount of industrial loans and Phase-3 districts the largest. This is on expected lines since Phase-1 districts were the poorest and least-industrialized districts of the country. Industrial loans were also the lowest fraction of total bank credit in Phase-1 districts (compared to Phase-2 and Phase-3 districts). Agricultural loans, on the other hand, formed the biggest fraction of lending in Phase-1 districts. Although the value of agricultural loans made in Phase-1 districts was still lower than in the richer Phase-3 districts.

It is clear from the summary statistics that the three groups of districts were fundamentally different. Since we employ a difference-in-differences estimation strategy, it is important to explore in detail the identifying assumptions, especially where the 'treatment' and 'control' groups are different in characteristics. In the following section, we discuss the estimation strategy and spell out the exact assumptions that are required for us to identify the parameters of interest.

3.2 Empirical Strategy

Our objective in this paper is to measure the impact of the implementation of NREGS on bank lending through the formal banking channels in rural India. Our identification rests on the staggered implementation of NREGS to employ the staggered difference-in-differences (DiD) estimation procedure. The staggered DiD has been the subject of criticism in the recent econometrics literature. We, therefore, explicitly mention all the assumptions that are needed to identify the average treatment effects on the treated (ATTs) and discuss the plausibility of those identifying assumptions.

Mahatma Gandhi National Rural Employment Gurantee Scheme (NREGS) was implemented in a staggered manner. The Act was passed in the Indian Parliament in August 2005. The scheme came into force in February 2006 in the first 200 districts. An additional 130 districts received the program in April 2007, and all remaining rural districts started the program in April 2008.

We use the pre-treatment values of the 3rd group of districts as a control group. Thus, our sample has to end in 2008, after which we have no control group left for comparison. Given that the 3rd group is the control we can only measure the ATTs for the first two group of districts (Phase 1 and Phase 2 districts). We discuss below the the main identifying assumptions needed to consistently estimate the ATTs for those two groups.

To explain our assumptions we use the notation commonly used in the causal inference literature. $Y_t(0)$ denotes the potential outcome in period t of being untreated. Y in our study are variables such as stock of deposits and stock of different categories of credit. $Y_t(g)$ is the potential outcome in period t of being first treated in period g (and remain treated thereafter in future time periods). Thus for any $t < g$, a unit is yet to be treated. At any time period t, we observe either $Y_t(0)$ or $Y_t(g)$ but never both, which is the classic missing variable problem of causal inference.

In our setting our control group consists of those districts which received treatment in April 2008. We therefore observe $Y_t(0)$ for t from 2002 to 2008 for this group.[8] The two treatment cohorts are those that receive treatment in 2006 and 2007. Thus we observe in the data $Y_t(g)$ from 2002 to 2008, where $g \in \{2006, 2007\}$.

We want to estimate the ATTs for the cohorts defined as

$$\tau_t = E[Y_t(g) - Y_t(0) \mid d_g = 1]$$

where d_g is an indicator of whether a district belongs to the cohort g. In our setting, we can only estimate the τ_t for $t = \{2007, 2008\}$ when $g = \{2006\}$ and $t = \{2008\}$ when $g = \{2007\}$.

Assumption 1: No Anticipation Assumption

The first assumption needed to consistently estimate is that of no anticipation:

$$E[Y_t(g) - Y_t(0) \mid d_g = 1] = 0, \forall t < g \tag{1}$$

implying that there are no pre-treatment effects (nor any effects because of anticipation). This assumption may require some discussion because although districts were assigned to their respective year of implementation, the outcome variables of deposit and lending may have been influenced by anticipation of the program. One way to address this concern would be to remove the data for the years between the announcement and implementation of the program, and estimate the treatment effects. The NREGS program was announced in 2005. In section 5, we test the robustness of our results to this assumption by dropping observation for the years 2005 and 2006. What however remains a counterfactual, and cannot be tested, is whether there were any anticipatory effects in the Phase 2 and Phase 3 districts *after* implementation of the scheme in Phase 1 districts.

Assumption 2: Common Trend Assumption

[8] The observations for 2008 are recorded in the end of March 2008, and hence are still pre-treatment.

The unconditional version of the second assumption formally states that for $t > 2002$

$$E[Y_t(0) - Y_{t-1}(0) \mid d_g = 1] = E[Y_t(0) - Y_{t-1}(0)] \tag{2}$$

where d_g is an indicator of whether a district belongs to the cohort g. The assumption essentially states that the difference between the potential outcomes of two different time periods (in the untreated state) is independent of treatment assignment. This is more popularly know as the "parallel trends" assumptions. That is, in the absence of treatment, the expected *change* in the outcome variable across periods is the same for all cohorts.

For the purposes of our empirical exercise the above assumption may be a strong one because the basis on which the districts were divided into phases of implementation were based on their socio-economic status. Thus to assume that the outcome variables would trend similarly in the absence of NREGS would be unwise. However, the government of India created a distinct formula based on which the assignment schedule was fixed for the districts. The government used a two-stage assignment algorithm to allocate districts to implementation phases: In the first step, each Indian state received a quota of treatment districts proportional to the percentage of India's poor living in that state. In the second step, the quota in each state was filled with the poorest districts according to a development ranking. This setup was designed to ensure inter-state fairness in the allocation of districts in the first step and intra-state fairness in the second step.

The government provides development ranking for 447 districts based on 3 indicators; *Total Scheduled Caste & Tribe Population percentage*, *Agricultural Wages* and *Agricultural Productivity per Worker*.[9] We therefore limit our analysis to these 447 districts and restate our assumption 2 as follows:

$$E[Y_t(0) - Y_{t-1}(0) \mid d_g = 1, \mathbf{X}] = E[Y_t(0) - Y_{t-1}(0) \mid \mathbf{X}] \tag{3}$$

which essentially states that, conditional on the pre-treatment co-variates, $\mathbf{X}$, the potential outcomes in the untreated state trend in a similar manner.[10]

Following (Wooldridge (2021)), the estimating equation with the conditional common trends assumption is as follows:

$$\begin{aligned} Y_{dt} = \alpha_d + \gamma_t + \beta_1 Phase_1 \times t_{2007} + \beta_2 Phase_1 \times t_{2008} + \beta_3 Phase_2 \times t_{2008} \\ + \mathbf{\Omega} \sum_g \sum_{t=g+1}^{2008} Phase_g \times t \times (\mathbf{X} - \mu_{\mathbf{1}}^{\mathbf{g}}) \\ + \mathbf{\Phi} \sum_t t \times (\mathbf{X}) + \epsilon_{dt}, \end{aligned} \tag{4}$$

where d indexes districts and t indexes years. μ_1^g is the group specific mean ($g \in \{2006, 2007\}$) of the pre-

[9]The study was conducted by a task force of the Planning Commission of the government and published in 2003.

[10]Similarly, instead of assumption 1, we can use the assumption of conditional no anticipation. That is, $E[Y_t(g) - Y_t(0) \mid d_g = 1, \mathbf{X}] = 0, \forall t < g$. However, this isn't much of a change because conditional no anticipation implies unconditional no anticipation, because $E[Y_t(g) - Y_t(0) \mid d_g = 1] = E\{E[Y_t(g) - Y_t(0) \mid d_g = 1, \mathbf{X}] \mid d_g = 1\} = 0$.

treatment covariates $\mathbf{X}$. This centering is needed such that the βs can be interpreted as the ATTs (Słoczyński, 2022). The estimating equation can be consistently estimated using the Two-Way Fixed Effects OLS estimator and would provide dynamic treatment effects, i.e., treatment effects varying by group, time, and covariates. $Phase_1$ is the first group of districts which receive treatment in 2006 and $Phase_2$ is the second group of districts which receive treatment in the second phase in 2007. The third group (which received treatment in 2008) is the control group. After accounting for delimitation issues, as discussed in the previous section, we are left with 420 districts out of the 447 for which we have the co-variates data.

β_1 estimates the ATT for the first group of districts (those where the program was implemented in 2006) in 2007 and β_2 estimates the effect of this group in 2008. Thus, we allow the treatment effects to vary over time. β_3 estimates the ATT of the second group of districts (receiving treatment in 2007) in 2008. Note that we allow the treatment effects to be heterogeneous across both cohort and time (and covariates too). We do not impose any restrictions on the dynamics of the treatment effects. Imposing strict restrictions may lead to faulty inferences, a point that has been made by the growing literature on difference-in-differences (Callaway and Sant'Anna (2021), Sun and Abraham (2021), Borusyak, Jaravel, and Spiess (2022), Goodman-Bacon (2021); see de Chaisemartin and D'Haultfœuille (2022) and Roth et al. (2022) for a recent review).

Although the above two assumptions are considered the standard assumptions in difference-in-differences, there remain two additional assumptions that are still necessary (and often implicit in applied studies) for estimation. We explicitly mention those as follows and discuss them in the context of our setting.[11]

Assumption 3: Stable Unit Treatment Value Assumption

Another assumption that often goes implicit in causal inference studies is that of stable unit treatment value. This basically assumes that the treatment of one object has no effect on other objects. For empirical analyses where the unit of focus is a district, this assumption may be a strong one. This is because the treatment of one district may have spillover effects on other neighboring districts. Given the nature of our data, it is not possible to test this assumption with reasonable statistical power. However, we provide some anecdotal evidence in Section 4 in support of this assumption.

Assumption 4: Overlap Assumption

The final identifying assumption needed to estimate the treatment effects is the overlap assumption. The assumption states that

$$P(d_g = 1 \mid \mathbf{X} = \mathbf{x}) < 1, \quad \{\forall\ \mathbf{x} \in supp(\mathbf{X})\ ;\ g \in \{2006, 2007\}\}, \tag{5}$$

where P is the probability of being in a particular cohort, conditional on covariates. The move from the stronger unconditional common trends to the weaker conditional common trends comes at a cost. Since we now have covariates to condition on, we must be certain that the counterfactuals for the several cohorts are

[11]Technically, imposing a linear structure in Equation 4 is also an additional assumption. It is possible to estimate treatment effects in a completely non-parametric setting, without imposing any structure, using kernel estimators. We, however, have not adopted that research design for the purposes of this paper.

estimable using the covariate data. Loosely speaking, if certain values of covariates perfectly predict which cohort a district will be in, then we cannot hope to estimate any counterfactuals for those values of covariates because no data exists. That is why this additional assumption is needed to ensure viable estimation of the estimands of interest.

4 Results

4.1 Overall Impact on Credit Outstanding

Table 2 reports the results of the difference-in-differences estimator of heterogeneous treatment effects (i.e., varying by cohort and time). Columns (1) employs the stronger assumption of unconditional common trends. Whereas, column (2) is estimated under the weaker conditional (on pre-treatment covariates) common trends assumption (as discussed in Section 3). Under the conditional common trends assumption (column (2)), we find loans outstanding, for Phase 1 districts, of around 21.8% in the year immediately after the NREGS program was implemented. In the subsequent year, phase 1 districts witnessed an even further increase of 22.4%. Phase 2 districts however did not witness any substantial increase in loans after the implementation of the program. The point estimate for Phase 2 was a moderate 3.3% but statistically insignificant.

[Insert Table 2 here]

Under the unconditional common trends assumption (columns (1)) we do not find any statistically significant effects of the program on credit outstanding. We refrain from drawing any inference from this estimation as this assumption is unlikely to hold. The earlier districts where the program was implemented were distinctively different from the later districts and the trend of deposits and loans, unconditional on economic fundamentals, would be different. We report the results to serve as a contrast of how the estimates change when moving from one identifying assumption to another.

In Figure 1, we plot the ATTs for Phase 1 and Phase 2 districts from 2003 on-wards.[12] The event-study analysis corroborates the previous findings. Phase 1 districts saw a clear increase in loans outstanding. No such effect was noticed for Phase 2 districts.

[Insert Figure 1 here]

Why does the employment guarantee program increase borrowing for the early phase districts? There are several mechanisms through which the NREGS program could have increased borrowing. In the subsequent sections we explore several mechanisms and provide empirical evidence which will help us either corroborate or reject such mechanisms.

[12]This is modified event-study analysis where we the use the 1st year of observations (i.e., 2002) as a reference to plot estiamtes of ATTs for subsequent years. The estimation utilizes the conditional common trends assumption and takes care of the 'contamination' of weights as discussed in Sun and Abraham (2021).

4.2 Impact of NREGS on Household Credit

We first explore whether the employment guarantee program increased the total amount loans taken by households located in Phase 1 and Phase 2 districts. The guaranteed employment provides each household with a fixed and uncertain income every year. This potentially increases the households' marginal propensity to consume. The impact on household borrowing is not immediately clear. On one hand, since low-income households often borrow to maintain living standards when redistributive mechanisms are limited (Kumhof, Rancière, and Winant (2015)), the introduction of the scheme could potentially lead poor households to borrow less. On the other hand, the increased and certain income could induce households to borrow and smooth consumption.

Before we analyze the results on household borrowings from banks, it is important to understand what "borrowing" means for poor households, in the Indian context. Poor households in India have very limited access to formal banking systems (Burgess and Pande (2005)). These households, which are rationed out of the formal credit market, borrow from "informal sources" such as money lenders, pawn brokers, etc. The intermediaries in turn borrow from banks (Surendra (2020)). It is not possible to observe "informal borrowing" in an annual frequency.[13] We do, however, observe the borrowing by the mentioned intermediaries in the banking data. Thus, we complement our analysis of personal borrowing by also exploring bank borrowings of financial intermediaries.

Using the same empirical strategy as earlier we analyze the impact of NREGS on *Personal* loans and loans of *other financial institutions* (which for us proxies for "informal sources", that in turn where providing credit to poor households). We report the results in Table 3. In Appendix 1, we define what these loan categories entail.

[Insert Table 3 here]

We note in Table 3 and Figure 2 that *Personal* loans saw a 19.7% increase in year 1 and 27.0% increase in year 2 for Phase 1 districts. Personal loans include mortgages, loans for consumer durables, auto loans, educational loans, etc. Since personal loans, on average, constitute about 1/4th of loans in any district, the increase in personal loans contributed significantly to the growth in overall lending after NREGS in Phase 1 districts. The growth in personal loans in Phase 1 districts suggests that households in the poorest districts of India found it optimal to borrow, possibly to increase immediate consumption, or to increase investment in social capital such as education. Banks, in turn, found lending to these households profitable, given the safety-net of guaranteed employment.

[Insert Figure 2 here]

It is important to contextualize these findings in relation to how the rural economy of the country is characterized. The Indian economy is often, rather simplistically, characterized to have dual components:

[13]National surveys in India collect this data, but biennially, or in some cases quinquennially.

'*formal*' and '*informal*'. The *informal* economy is larger in size, is primarily cash driven, and has limited access to formal channels of banking. The job guarantee program was likely to benefit households/individuals who were primarily associated with the *informal* economy. Thus it may be difficult to estimate effects of the program on financial intermediation by focusing only on formal channels of credit. However, one defining feature of the scheme was that payments to labourers had to be made only via bank accounts. Thus, those participating in the program were forced to come within the ambit of formal banking channels. Hence, even though NREGS was essentially a safety-net for the informal economy, the effects of the same can potentially be estimated from the data of scheduled banks.

We note a large increase in the stock of Financial loans in Phase 1 district after the first year of implementation. Financial loans increased by 21% after one year of implementation (although this is not statistically significant). Financial loans include loans by banks to other financial intermediaries, including loans to regional rural banks, pawn brokers, private money lenders, etc. Since, the reach of commercial banks in providing banking services in the rural districts of India has been limited and that role has been met by informal/semi-formal channels of credit. The onset of the job guarantee program potentially led to larger borrowing by households, who borrowed from smaller intermediaries more prevalent in rural areas. These smaller intermediaries in-turn borrowed from banks to meet the higher credit demand.

Although, the magnitude of increase in financial loans is remarkable, we are circumspect of drawing conclusions from these estimates for two reasons. First, the contribution of financial loans to the growth of overall loans is small. This is because financial loans form only a small fraction (0.6%) of total loans outstanding in any district (seen in Table 1). Second, the later robustness checks demonstrate that these estimates are not stable, and exhibit large standard errors and fluctuating coefficients.

Lastly, we do not find any meaningful and significant effect of NREGS on Agricultural loans. This is expected to some extent given the credit outstanding in the BSR dataset is computed annually, net of all loan repayments. Most agricultural loans are seasonal and therefore repaid within the year. Thus, the program's impact on agricultural loans may not be visible due to the nature of the BSR data.

We do not find evidence that NREGS had an impact in household borrowing of Phase 2 districts. These districts by design were relatively better off than Phase 1 districts. The lack of evidence in Phase 2 districts is informative in the sense that it helps us narrow down on potential mechanisms. Any explanation of such mechanisms has to account for this lack of evidence. For instance, if the increased borrowing were driven by consumption smoothing arguments of households, then one is hard pressed to explain why no such effects are observable for Phase 2 districts. We thus hypothesize, that more than demand, the increase in household borrowing in Phase 1 districts was a consequence of reduce information asymmetry for banks. We explore this line of reasoning further.

[Insert Figure 3 here]

Since NREGS used formal banking channels to disburse wage payments, the program brought large fraction of the 'unbanked' rural population within the scope of banking system. We show evidence of this by focusing on the number of accounts in rural areas. In Table 4 and Figure 3, we report the results of our analysis of bank accounts in rural areas and total deposits. We estimate that NREGS caused a 8.6% increase in bank accounts in the first year and around 8.6% in the 2nd year after policy implementation for the Phase 1 districts. We again don't find any significant effect on Phase 2 districts.[14]

[Insert Table 4 here]

We argue that the increase of access to banking services, as evidenced by increase in bank accounts, was the key driver of increased household borrowing. There are evidences of real effects of expansion of formal banking systems on economic outcomes both in the context of India (Burgess and Pande (2005), Burgess, Wong, and Pande (2005), Kochar (2018)), and other developing nations (Bruhn and Love (2014), Karlan and Zinman (2010)). We argue that such expansion of banking services, a direct outcome of NREGS, had significant impact on household borrowing.

The social safety net aspect of the scheme provided a stable source of income for this new customer base (Oldiges, 2015). In addition, the new customers had proper identification for tracking and building a banking relationship with the new clients. This costly investment in the state's fiscal and legal capacity benefited the financial system by substantially improving their *verification* capabilities (Besley and Persson, 2011). In this sense, the welfare state worked complementary to credit markets.

An increase in formal bank accounts by itself may not have increased formal credit uptake. It's plausible that the previously untapped segment of rural borrowers were already being served by the informal credit market (local moneylenders or relatives). The observed increase in formal bank credit, without any change in the interest rate, could be an equilibrium response to a reduction in information asymmetries. In section 6, we present a modified version of the Stiglitz and Weiss (1981) model for formal exposition of the mechanism. Intuitively, the mechanism relies on the idea that when credit is rationed, then reducing the information asymmetry between banks and borrowers can increase equilibrium amount of loans, without effecting the price of credit.

The findings help broaden our understanding of whether welfare schemes complement or substitute credit markets. We find evidence that the relation can be nuanced and primarily depends on the nature of welfare support and the structure of credit markets. This finding is consistent with other scholars who document similar evidence of this complementarity. When a welfare scheme is designed such that it directly impacts the credit regime in a country, then credit markets can indeed complement welfare schemes by giving participants access to credit.

4.3 Impact of NREGS on Firm borrowings

Table 5 reports the results of the difference-in-differences on loans to industries, professional services, and trade services (refer to Appendix 1 for definition of these categories). We estimate that NREGS led to

[14] On deposits we find a rather small effect in the first year (increase of 7%), but no effect in the 2nd year. The results on deposits, however, do not survive robustness tests.

an increase in industrial loans of around 20% (column 2) in the first year and around 19% in the 2nd year (albeit statistically insignificant) in Phase 1 districts. Industrial loans primarily include manufacturing units, which for the poorer Phase 1 districts are small and medium enterprises. For Phase 2 districts we do not find any discernible increase in industrial loans. In fact, the point estimate for industrial loans are negative, but insignificant. Industrial loans constitute, on average, 28% of loans outstanding in a district. Thus a 20% increase in industrial loans contributed significantly to the growth of total loans outstanding in the first year after NREGS. We report the evolution of the treatment effects in Figure 4.

[Insert Table 5 here]

Why were industrial firms in Phase 1 districts borrowing more because of NREGS? One explanation is related to the mechanism of increased consumption from households. Because consumers increased demand, firms borrowed to make investments to increase production capacity. Although it is hard to pin down this mechanism, the increase in production capacity to keep up with the increased demand would certainly lead to increased sales and turnover for firms in the treated districts. However, firms in treated districts did not report increased sales (Agarwal et al. (2021)). Therefore, this hypothesized mechanism is not convincingly corroborated by data.

Another explanation is that since NREGS increase private sector wages (Imbert and Papp (2015)), firms faced increased costs of production. Firms in response moved towards less labour intensive technologies and borrowed more to make investments in such technologies.Agarwal et al. (2021) find evidence that after NREGS firms faced adverse labour-supply shock and resorted to increased mechanisation. Our findings of increased industrial loans sit consistent with their evidence of increased firm investments and mechanisation.

[Insert Figure 4 here]

Next, we note Trade services witnessed an increase in credit uptake of around 18%, in both the years, because of NREGS. Trade services include loans to wholesale and retail establishments. An increase in their borrowing is suggestive of these establishments either expanding operations or increasing inventory. This finding is consistent with the hypothesis that NREGS increased the capacity of households to consume, which in turn caused retail and wholesale establishment to borrow more.

5 Alternative Estimates & Robustness Checks

In our baseline results, we find most of the effects from NREGS on Phase 1 districts and limited effects on Phase 2 districts. In this section, we present estimates using alternative DiD techniques or by relaxing some DiD assumptions. We find similar results to our baseline results reported before.

5.1 Aggregate Effects

[Insert Table 6 here]

Here, we redo the analysis to find the aggregate group-wise treatment effects. Table 6 reports the results where we estimate the treatments effects only by treatment cohort (and no variation of treatment effect by time). The results are similar to those reported in Section (4). Phase 1 districts saw an average increase in lending by around 9.4% which was led by increases in industrial lending (increase of around 20%), personal lending (increase of around 9.4%) and, increase in loans to trade services (approximately 11%). Financial loans also increased massively by around 35%, but since financial loans constitute only 0.6% of loans in phase districts, they are unlikely to have contributed to the overall increase in credit in those districts.

5.2 Relaxation of No Anticipation

Next, to alleviate concerns with anticipation we redo our analysis by omitting data for the year of the announcement but before the implementation of NREGS (i.e., 2005 and 2006). We report the results in Table 7. Note that this analysis only addresses the concern with anticipatory effects of the program for *all* districts in the years before Phase 1 implementation. It does not address the issue that after Phase 1 implementation, Phase 2 and Phase 3 districts could have witnessed anticipatory effects before the program started in those districts.

[Insert Table 7 here]

Here, too, we find the results similar to the baseline results. For example, Phase 1 districts witnessed a growth in loans of about 15% in the 2nd year, which were primarily lead by increases in industrial and personal loans. Similarly, we find loans to trade services witnessed a growth of almost 19%. We note that these point estimates are larger in magnitude than some of the baseline results. This suggests that there may have been some anticipatory effects which were attenuating the estimated effects in the baseline regressions.

5.3 Alternative Estimator of the Staggered Difference-in-Differences

In this section we use a different estimator to estimate (a variant of) Equation 4. We use the Callaway and Sant'Anna (2021) approach to estimate the effects. We report the results in Table 8. The results are in line with our TWFE estimator. In fact, the estimates are larger and significant for several additional categories (such as Agriculture). But we refrain from drawing any conclusion on these categories, as the results are not robust to other estimation techniques.

[Insert Table 8 here]

5.4 Synthetic Difference-in-Differences

[Insert Table 9 here]

Our results thus far indicate that NREGS had an impact on Phase 1 districts. We were unable to find any significant evidence on Phase 2 districts. In this section we redo our analysis by dropping Phase 2 districts from our sample. This allows us to move to the standard difference-in-differences setting with 1 treatment group (Phase 1 districts) and 1 control group (Phase 3 districts). We then compare those results with an alternative estimation using synthetic difference-in-differences (Arkhangelsky et al. (2021)).

The synthetic control method uses a long panel data to construct a weighted average of the control group units that closely match the observable characteristics of the treatment group over time in the pre-treatment phase. The treatment impact is then a simple difference between the treatment and (synthetic) control group or cohort. The synthetic control method does better than the standard DID in bringing an ideal control group that best matches the treatment group in observable attributes. However, the synthetic control method has time-weight issues away from the pre-treatment period due to the in-built linear model assumption. Synthetic DID can be interpreted as a unit and time-weighted variant of the standard DID estimator. It is consistent under a range of weighting schemes when the DID model is correctly specified. It is also consistent with appropriate synthetic control weights under certain weak conditions.

In Table 9, we estimate the standard difference-in-differences estimator. The results are in line with our staggered approach. That is, personal loans and industrial loans witnessed a marked increase because of NREGS. In Table 10, we redo the difference-in-differences, but this time using synthetic controls. We compare the estimates of the two different approaches in Figure 5. Although statistically significant, the coefficients on total loans, personal loans, industrial loans are markedly smaller when estimated using the synthetic difference-in-differences approach. One interpretation of the results is that the control group (Phase-3 districts) is less close of a match with the treated group (Phase-1 districts) as one could achieve with synthetic control methods. In that sense, one can interpret the Synthetic DID results as a lower bound of the effect of NREGS on household and firm credit.

[Insert Table 10 here]

[Insert Figure 5 here]

6 Reduction of information asymmetry increases credit supply

In this section, we formally develop the argument that decreasing information asymmetry increases credit supply *without* impacting the price. The absence of institutions in developing countries for effective screening of borrowers (to mitigate adverse selection) or ongoing monitoring of borrowers' actions (to mitigate moral hazard) has played a significant role in financial underdevelopment. Acute information asymmetry could result in credit rationing, with extreme cases leading to absence of credit markets altogether (Akerlof (1970)). In that context, we use insights from the model of credit rationing (Stiglitz and Weiss (1981)) where lenders cannot distinguish ex-ante between high-quality and low-quality borrowers – those that will repay their loans and those that will default. We modify the model to show that a decrease in information asymmetry will increase bank credit supply.

6.1 The model setup

The rural credit market setting Let there be many banks and many (potential) borrowers. Banks maximize profits through their choice of interest rate, collateral, and mix of projects.[15] Borrowers maximize profits through their choice of project. Depositors satisfy the zero-profit condition. There is competitive banking in the sense that all banks maximize profits, even though banks may not be price-takers. We assume that without relationship banking and identification paperwork with the borrowers, many rural projects are observably indistinguishable to bankers for lack of information about the projects' odds of success or the borrowers' actions. We will show first that this could potentially lead to a bank credit supply that is less than market demand, i.e., a credit rationing equilibrium.

Projects are marked by risky returns Borrowers are characterized by their projects (γ) which have a risky return (R). Let each borrower have a single project in a time period. The projects differ on returns. Borrowers wish to finance part of their projects from banks. Banks wish to finance projects with higher mean returns but lower risk. We simplify the bank's decision problem to projects with the same mean return. We assume that banks can distinguish between projects based on mean returns. But they cannot identify the spread of returns, i.e., projects with the same mean but different risk profiles are indistinguishable.

[Insert Figure 6 here]

Let each project of risk type γ be associated with a cumulative distribution function, $F(R, \gamma)$, of returns on the project. We use higher values of γ to represent a higher risk: $\gamma_2 > \gamma_1$ implies $F(R, \gamma_2)$ is a mean-preserving spread of $F(R, \gamma_1)$ (second-order stochastic dominance (Rothschild and Stiglitz, 1978)). In other words, while the mean return is the same for both γ_2 and γ_1, a risk-averse agent will prefer project γ_1 over project γ_2. For ease of exposition, we assume that every return is equally likely, i.e., a project's returns are distributed over a uniform distribution (see Figure 6 for a Uniform probability distribution function of returns, $f(R, \gamma)$). The results are generalizable to any distribution of returns.

[Insert Figure 7 here]

[15] In a rural setting with limited liability, there is usually no possible collateral. But we show the results work even if there is collateral.

Distribution of projects Let the distribution of risk types γ be given by cumulative distribution function $G(\gamma)$, probability distribution function $g(\gamma)$ over the support $\gamma \in [\underline{\gamma}, \overline{\gamma})$. As mentioned before, higher γ projects pose greater risk. See Figure 7 where we assume that the distribution G is normal.[16] [17]

[Insert Figure 8 here]

Bank's profit The return to the bank on any project (γ, R), for given interest rate, r, and collateral, C, on loan amount, L, is given by:

$$\rho(R, r) = min(R + C; L(1 + r))$$

Until the project earns a return higher than what was promised to the bank ($L(1 + r)$) net of collateral C, the bank gets less than the full amount promised. In this case, we assume that the bank's return is proportional to the project's realized return. We plot the bank's return as a function of the project's realized return in Figure 8. Note from the figure that the bank's return is concave in R. Thus, the certainty equivalent of the project return to the bank is decreasing in a bigger spread of R, i.e., a higher γ. Let the certainty equivalent of project return be denoted as $E\rho(R, r)$.

For any given interest rate, the expected return, $E\rho(R, r)$, to the bank is decreasing in the riskiness of the project. See Figure 9, where the bank's expected profit $E\rho(r)$ is a decreasing function of project risk γ at a given interest rate.

[Insert Figure 9 here]

Borrower's profit The borrower's return to a financed project is given by

$$\pi(R, r) = max[R - L(1 + r); -C]$$

where R is the project's realized return, L is the loan amount to be repaid to the lender, r is the agreed upon interest rate, and C is the collateral. We note two things regarding the borrower's return function. First, the borrower's return is convex in R. The certainty equivalent for the borrower increases with risk (bigger spread of R). This is in contrast to the lender's return to risk. Second, the borrower's profit (π) decreases with higher interest rate. As the interest rate increases, fewer projects will find it profitable to seek financing on the margin.

The marginal borrower We know that the borrower's return is convex in R and decreasing with the interest rate. These two points together imply that at any given interest rate $\hat{r}$, there will be a threshold of risk-type $\hat{\gamma}$ for whom expected profits from financing will be zero. Only riskier projects ($\gamma > \hat{\gamma}$) will

[16] Again, results do not rely of the type of distribution.

[17] These are subjective distributions - the borrower's perception of returns could be different from the lender's.

demand credit to profitably finance the project. Additionally, it means that an increase in the interest rate will increase the risk-threshold of projects seeking finance.

$$\frac{d\hat{\gamma}}{d\hat{r}} > 0$$

Proof. For any given interest rate $\hat{r}$, let the threshold risk type for which expected profits are zero be $\hat{\gamma}$. Then, $\hat{\gamma}$ satisfies the following condition:

$$\Pi(\hat{r}, \hat{\gamma}) \equiv \int_0^\infty max[R - L(1 + \hat{r}); -C] dF(R, \hat{\gamma}) = 0$$

Differentiating the above equation completely and rearranging, we get

$$\frac{d\hat{\gamma}}{d\hat{r}} = \frac{L \int_{(1+\hat{r}L-C)}^{\infty} dF(R, \hat{\gamma})}{\partial\Pi/\partial\hat{\gamma}} > 0$$

□

In Figure 9, $\hat{\gamma_1}$ is the marginal risk borrower associated with the interest rate $\hat{r_1}$. Similarly, let $\hat{\gamma_2}$ be the marginal risk borrower associated with the interest rate $\hat{r_2}$. As the interest rate increases ($\hat{r_2} > \hat{r_1}$), the threshold risk borrower also shifts right from $\hat{\gamma_1}$ to $\hat{\gamma_2}$ ($\hat{\gamma_2} > \hat{\gamma_1}$).

6.2 Establishing an equilibrium with credit rationing

[Insert Figure 10 here]

Credit supply is driven by bank profits Higher profits allow a bank to offer better rates to depositors, attracting more deposits. Thus, the bank's credit supply is directly affected by the profitability of its loan portfolio. We show this in Figure 10, where $\overline{E\rho}$ is the mean of expected return over the range of projects in the bank's portfolio in Figure 9 (the midpoint of the $E\rho$ curve over the probable risk portfolio).

The bank could increase profits through two channels - first, by raising interest rates, and second, by reducing the riskiness of its risk portfolio. We address the feasibility of these channels sequentially below.

Direct effect of increase in interest rate A higher interest rate, r, increases the bank's return from each project, all else equal. See Figure 9, where the bank's expected profit is a decreasing function of project risk. We show the effect of an increase in interest on bank's profitability by shifting the bank's profit curve to the right.

Increasing interest rates lead to adverse selection or moral hazard The bank's ability to raise interest rates (without reducing profits) is limited by adverse selection and/or moral hazard (Stiglitz and Weiss, 1981). This is because at a given interest rate $\hat{r}$, there exists a threshold level of risk-type, $\hat{\gamma}$, such that less risky projects ($\gamma < \hat{\gamma}$) don't demand credit as it's too expensive for them. If the bank raises interest rate for potential borrowers, safer projects will drop out of the market (adverse selection). Among existing borrowers who have already received credit, a firm is more likely to pick the riskier project between two projects when interest rates are hiked (moral hazard). Either way, interest rate increases have an indirect negative impact on the bank's profitability (asset substitution in finance). In Figure 9, we can show this by limiting γ from the left. The new riskier portfolio has a lower mean return to the bank, $\overline{E\rho}$.

[Insert Figure 11 here]

r's net effect on bank profitability The direct effect of raising interest rate on lender's profits is positive. However, the indirect effect through the risk portfolio selection is negative. In Figure (11), we plot the lender's net expected profit as a function of the interest rate. We see that for low r, the direct (positive) effect dominates. However, for higher interest rate levels, the indirect effect starts to dominate. Thus, the lender's profit is a non-monotonic function of r. This non-monotonic relationship gives the optimum interest rate ($\hat{r}$) for a given risk portfolio ($\gamma > \hat{\gamma}$).

Credit rationing equilibrium The bank's profit-maximizing rate of interest ($\hat{r}$) may be lower than the market-clearing rate, say r^*. Whenever $r^* > \hat{r}$, there is equilibrium credit rationing. Figure 12 illustrates the case using a supply-demand framework.

[Insert Figure 12 here]

6.3 Analyzing NREGS: Reducing information asymmetry reduces credit rationing

Limited liability upon default means that lenders' payoff is a concave function of the project's return while the borrowers' function is convex. When the interest rate is hiked, the demand for loans comes from increasingly risky borrowers, as the less risky types drop out of the market due to lower profit. Profit-maximizing lenders will never choose to raise the interest rate beyond where the adverse selection or moral hazard effect dominates. If excess demand exists in the market at this rate, a credit rationing equilibrium will follow.

An alternate way for a lender to increase profits, without affecting the interest rate, is by selecting a less risky portfolio. There are two ways a bank can achieve this. First, by lowering the bottom threshold of risk distribution, i.e., attracting the least risky types. However, this isn't possible without offering a lower interest rate. The second option is to screen out the maximum risk-type borrower from its portfolio. This would imply successfully identifying and eliminating the poorest risk-types.

Information control The strategy of developing better identification and verification capabilities is a costly but plausible way to increase profits without changing the interest rate. Such capabilities can allow the bank to not only screen the worst-risk types out of its portfolio, but also attract the better-risk types with a lower rate, increasing its expected profitability.

Bank's expected profit over its entire portfolio Average expected return to the bank are given by the conditional mean of the profit function over the range of risk-types $\gamma \in [\hat{\gamma}, \overline{\gamma}]$:

$$\overline{E\rho(\gamma,\hat{r})} = \frac{\int_{\hat{\gamma}}^{\overline{\gamma}} E\rho(\gamma,\hat{r}) dG(\gamma)}{G(\overline{\gamma}) - G(\hat{\gamma})}$$

where $\overline{\gamma}$ represents the extreme risk cases to which the bank lends.

Lemma 6.1. *$E\rho(\gamma,\hat{r})$ is a decreasing function of γ, i.e., bank's profit is inversely related to the riskiness of project γ.*

Corollary 6.1.1. *Lemma .1 implies that removing the worst-risk types from the bank's portfolio will improve the bank's profitability.*

Proof. In the Appendix □

Now consider that an exogenous shock increases the number of people with bank accounts and identification paperwork. The borrowers' interaction with the bank increases through the bank accounts. More interaction and IDs for verification increase the bank's information about the borrower and enables it to better identify the extreme cases of the risk distribution (reduced asymmetry of information between risk types and the bank). In other words, the unobserved risk-type space shrinks. As a result, the bank can confidently screen out the extreme risk (riskiest) type borrowers. The new conditional distribution can be represented by a lower upper-bound ($\tilde{\gamma} < \overline{\gamma}$) of the risk-distribution to which the bank lends (smaller type space of projects). Thus, less information asymmetry can allow the bank to focus its funds on the lower risk-type projects and raise its return $E(\rho)$ at the same interest rate $\hat{r}$. At any $\hat{r}$, the bank's profit increases. The supply of loanable funds (L_s) shifts up in Figure 12 to L_s'.

We posit that NREGS reduced information asymmetries in the Indian financial system by reducing the risk type-space relevant for the bank's risk portfolio. This was achieved through investments in state capacity such as identification cards and the expansion of banking infrastructure at the grassroots level, bringing more vulnerable population within the ambit of formal banking. In the data, we find a 10% increase in bank accounts in Phase-I districts relative to Phase-III districts, and a 20% increase in credit outstanding. Even after accounting for the increase on the extensive margin (number of bank accounts), we find an additional 10% increase on the intensive margin (amount of credit outstanding). Thus, costly investment by the welfare state in fiscal capacity and financial infrastructure that improves information verification capabilities may plausibly have led to a reduction in credit rationing.

7 Conclusion

In this paper, we analyze the impact of a major country-wide job guarantee program, launched in India in 2006, on financial intermediation. Exploiting the staggered implementation of the program we employ a difference-in-differences estimator to assess the treatment effect of the program on bank lending, deposits, and number of accounts. We find that the program led to an average of increase of 10% to 20% in bank loans for districts which received the treatment first. Industrial and personal loans are the two major categories where the impact was most significant. We do not find conclusive evidence of any effect on later treated districts. We also find a significant increase in bank accounts in early treated districts, but not in later ones. We rationalize our findings on personal loans through the lens of improved state capacity for program delivery supporting financial deepening in underdeveloped districts. We provide theoretical foundations for the relaxation of banks' credit supply constraints through a model of equilibrium credit rationing where the program reduced information asymmetries between banks and rural households. For industrial loans, we posit that the general equilibrium increase in private wages induced by potentially the program potentially led to a move towards labour-saving mechanization, financed by firms through increased bank borrowing.

Our analysis leaves open the question of whether the increase in bank's outstanding credit was a result of increase in demand, supply, or both. We are exploring this question in ongoing work. Another question raised by our work is whether overall borrowing went up or was it a switch from informal borrowing to formal. The two questions are related in some sense. It is unlikely that NREGS increased demand for personal credit on a scale large enough as is evident from our analysis. A more likely explanation is that the big increase in personal credit is additionally driven by loosening of bank credit supply constraints. Since formal credit is much lower in cost relative to informal sources, we hypothesize that the increase in personal credit is due to households getting improved access to lower-cost formal credit. On the other hand, the large increase in industrial credit could be due to an increase in overall demand for credit from firms to finance mechanization. However, since most firms are credit-constrained in India and bank credit is evidently rationed in developing countries (Banerjee, Cole, and Duflo, 2004; Banerjee and Duflo, 2010, 2014; Choudhary and Jain, 2022), it is not clear why or how banks increased supply. We believe further analysis is needed to help inform policymakers and researchers on the role of the welfare state in financial development.

Tables & Figures

Table 1

Summary Statistics

This table reports the summary statistics of the Basis Statistical Returns Data Provided by the Reserve Bank of India. Panel A summarizes all the districts in our sample. Panel B,C, D summarize the Phase 1, Phase 2, and Phase 3 districts in our sample. The observation level is district-year and the unit is billion INR.

Panel A: All Districts (N = 420)	N	% of Total Bank Credit	Mean	Std. Dev	Median	1st Quartile	4th Quartile
Total Bank Credit	2939	100.0%	9.63	14.20	5.10	2.39	11.02
Agriculture	2932	23.6%	2.28	2.51	1.48	0.65	2.97
Finance	2849	0.6%	0.06	0.35	0.01	0.00	0.03
Industry	2939	27.9%	2.69	6.09	0.72	0.23	2.59
Personal	2938	25.7%	2.48	4.54	1.17	0.51	2.74
Professional	2939	3.7%	0.35	0.78	0.12	0.05	0.34
Trade	2939	10.8%	1.05	2.24	0.54	0.29	1.04
Transport	2939	1.3%	0.12	0.37	0.04	0.02	0.11
Other	2845	6.2%	0.62	1.02	0.29	0.12	0.63
Deposits	2929	165.4%	15.99	21.49	9.75	5.23	18.38
Panel B: Phase 1 Districts (N = 163)							
Total Bank Credit	1140	100.0%	5.69	6.52	3.34	1.44	7.36
Agriculture	1137	27.4%	1.56	1.88	0.87	0.39	2.05
Finance	1084	0.6%	0.03	0.15	0.01	0.00	0.02
Industry	1140	23.2%	1.32	2.46	0.46	0.13	1.35
Personal	1140	26.1%	1.48	1.93	0.80	0.36	1.88
Professional	1140	2.8%	0.16	0.24	0.07	0.03	0.19
Trade	1140	12.0%	0.68	1.16	0.40	0.20	0.79
Transport	1140	1.2%	0.07	0.11	0.04	0.01	0.07
Other	1082	6.6%	0.39	0.54	0.20	0.07	0.43
Deposits	1131	184.7%	10.59	10.64	7.50	3.66	13.67
Panel C: Phase 2 Districts (N = 94)							
Total Bank Credit	658	100.0%	9.78	15.56	4.49	2.31	10.46
Agriculture	658	23.9%	2.33	2.89	1.34	0.70	2.79
Finance	636	0.6%	0.06	0.38	0.01	0.00	0.04
Industry	658	27.4%	2.68	5.87	0.50	0.21	2.17
Personal	658	24.4%	2.39	4.04	1.03	0.47	2.54
Professional	658	3.5%	0.34	0.69	0.11	0.04	0.31
Trade	658	12.5%	1.23	3.62	0.51	0.31	1.02
Transport	658	1.1%	0.11	0.17	0.05	0.02	0.12
Other	636	6.5%	0.66	1.11	0.26	0.12	0.61
Deposits	658	182.0%	17.80	29.48	9.34	5.41	17.65
Panel D: Phase 3 Districts (N = 163)							
Total Bank Credit	1141	100.0%	13.49	17.52	7.97	4.01	16.31
Agriculture	1137	21.9%	2.96	2.64	2.21	1.25	3.81
Finance	1129	0.6%	0.09	0.46	0.02	0.01	0.05
Industry	1141	30.1%	4.07	8.11	1.48	0.52	4.21
Personal	1140	26.1%	3.52	6.16	1.72	0.80	4.11
Professional	1141	4.1%	0.56	1.08	0.20	0.07	0.61
Trade	1141	9.7%	1.30	1.96	0.70	0.40	1.47
Transport	1141	1.4%	0.19	0.57	0.05	0.02	0.16
Other	1127	6.0%	0.82	1.25	0.42	0.19	0.89
Deposits	1140	150.4%	20.30	22.89	13.19	7.20	24.91

Table 2

Impact of NREGS on Credit Outstanding

This table reports the results of a Two-Way-Fixed-Effects staggered Difference-in-Differences estimation of the impact of implementation of the Mahatma Gandhi National Rural Employment Guarantee Scheme (NREGS) on total credit outstanding of scheduled commercial banks. The dependent variables are log values of the credit outstanding. NREGS was implemented in a staggered manner between 2006 and 2008. Phase 3 districts are the control group in our sample. In Column (2) the regression specification is given in Equation 4, whereas in columns (1) we use the stronger assumption of unconditional common trends, and hence does not include the covariates. The sample runs from 2002 to 2008. District boundaries are defined as per 2001 census. The standard errors are clustered at the district level.

	Credit Outstanding	
	(1)	(2)
$Phase_1 \times t_{2007}$	0.069	0.218***
	(0.049)	(0.075)
$Phase_1 \times t_{2008}$	0.074	0.224***
	(0.050)	(0.074)
$Phase_2 \times t_{2008}$	-0.027	0.033
	(0.060)	(0.065)
Constant	15.438***	15.181***
	(0.015)	(0.098)
Year FE	Yes	Yes
District FE	Yes	Yes
Demeaned Covariates	No	Yes
Observations	2939	2939
R^2	0.962	0.964

Figure 1

Treatment Effects over Time

This figure plots the average treatment effect on the treated, over time, for the Phase 1 and Phase 2 districts. The shaded area is the post treatment time period.

Table 3

Impact of NREGS on Personal, Financial, & Agricultural Loans

This table reports the results of a Two-Way-Fixed-Effects staggered Difference-in-Differences estimation of the impact of implementation of the Mahatma Gandhi National Rural Employment Guarantee Scheme (NREGS) on credit outstanding of scheduled commercial banks by categories. The dependent variables are log values of the credit outstanding of personal, financial, and agricultural loans. The regression specification is given in Equation 4. The sample runs from 2002 to 2008. District boundaries are defined as per 2001 census. The standard errors are clustered at the district level.

	Personal (1)	Financial (2)	Agriculture (3)
$Phase_1 \times t_{2007}$	0.197**	0.209	-0.009
	(0.087)	(0.410)	(0.067)
$Phase_1 \times t_{2008}$	0.268***	-0.593	0.043
	(0.095)	(0.392)	(0.072)
$Phase_2 \times t_{2008}$	0.037	-0.192	-0.048
	(0.075)	(0.306)	(0.064)
Constant	13.730***	10.190***	13.998***
	(0.142)	(0.511)	(0.113)
Year FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes
Demeaned Covariates	Yes	Yes	Yes
Observations	2938	2849	2936
R^2	0.964	0.710	0.972

Figure 2

Treatment Effects on Household Personal Loans over Time

This figure plots the average treatment effect on the treated, over time, for the Phase 1 and Phase 2 districts. The shaded area is the post treatment time period.

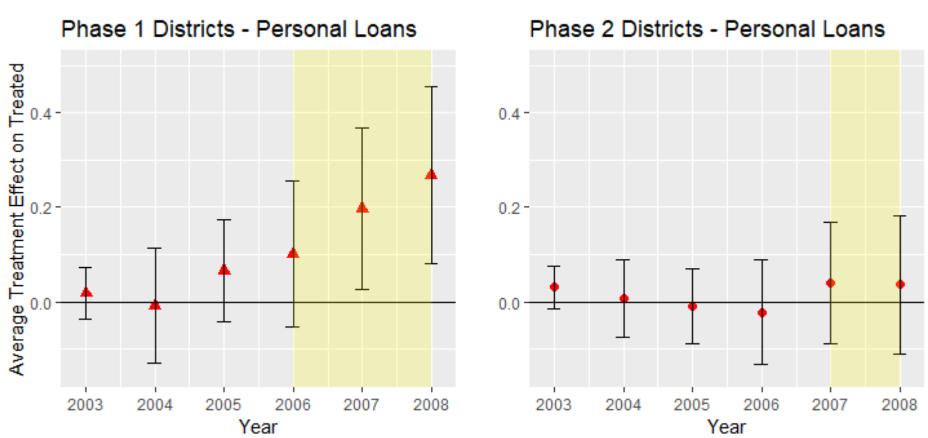


Table 4

Impact of NREGS Implementation on Bank Accounts and Deposits

This table reports the results of a Two-Way-Fixed-Effects staggered Difference-in-Differences estimation of the impact of implementation of the Mahatma Gandhi National Rural Employment Guarantee Act (NREGS) on total number of bank accounts and deposits. The dependent variables are log values of the number of bank accounts in rural areas in a district, and the log values of deposits. The sample runs from 2002 to 2008. District boundaries are defined as per 2001 census. The standard errors are clustered at the district level.

	Accounts (1)	Deposits (2)
$Phase_1 \times t_{2007}$	0.086^{**}	0.070^{*}
	(0.043)	(0.036)
$Phase_1 \times t_{2008}$	0.086^{**}	0.043
	(0.043)	(0.042)
$Phase_2 \times t_{2008}$	-0.002	-0.013
	(0.030)	(0.031)
Constant	12.112^{***}	16.150^{***}
	(0.045)	(0.058)
Year FE	Yes	Yes
District FE	Yes	Yes
Demeaned Covariates	Yes	Yes
Observations	2929	2929
R^2	0.987	0.992

Figure 3

Treatment Effects on Number of Bank Accounts over Time

This figure plots the average treatment effect on the treated, over time, for the Phase 1 and Phase 2 districts. The shaded area is the post treatment time period.

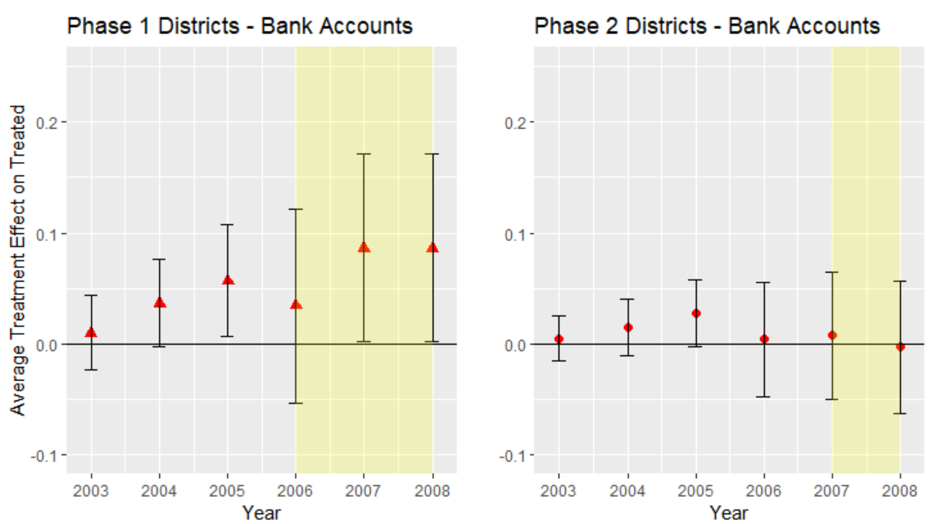


Table 5

Impact of NREGS Implementation on Loans to Firms

This table reports the results of a Two-Way-Fixed-Effects staggered Difference-in-Differences estimation of the impact of implementation of the Mahatma Gandhi National Rural Employment Guarantee Scheme (NREGS) on credit outstanding of firms. We are able to distinguish business establishments as industrial firms, a service firms, and trade firms. The dependent variables are log values of the credit outstanding of the respective categories of loans. The regression specification is given in Equation 4. The sample runs from 2002 to 2008. District boundaries are defined as per 2001 census. The standard errors are clustered at the district level.

	Service (1)	Industry (2)	Trade (3)
$Phase_1 \times t_{2007}$	0.146	0.206**	0.183*
	(0.155)	(0.097)	(0.098)
$Phase_1 \times t_{2008}$	0.160	0.187	0.176*
	(0.156)	(0.122)	(0.098)
$Phase_2 \times t_{2008}$	0.126	-0.067	0.024
	(0.115)	(0.089)	(0.066)
Constant	11.282***	13.218***	12.899***
	(0.217)	(0.193)	(0.143)
Year FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes
Demeaned Covariates	Yes	Yes	Yes
Observations	2939	2939	2939
R^2	0.913	0.915	0.924

Figure 4

Treatment Effects on Loans to Industrial & Trade Services Firms over Time

This figure plots the average treatment effect on the treated, over time, for the Phase 1 and Phase 2 districts. The shaded area is the post treatment time period.

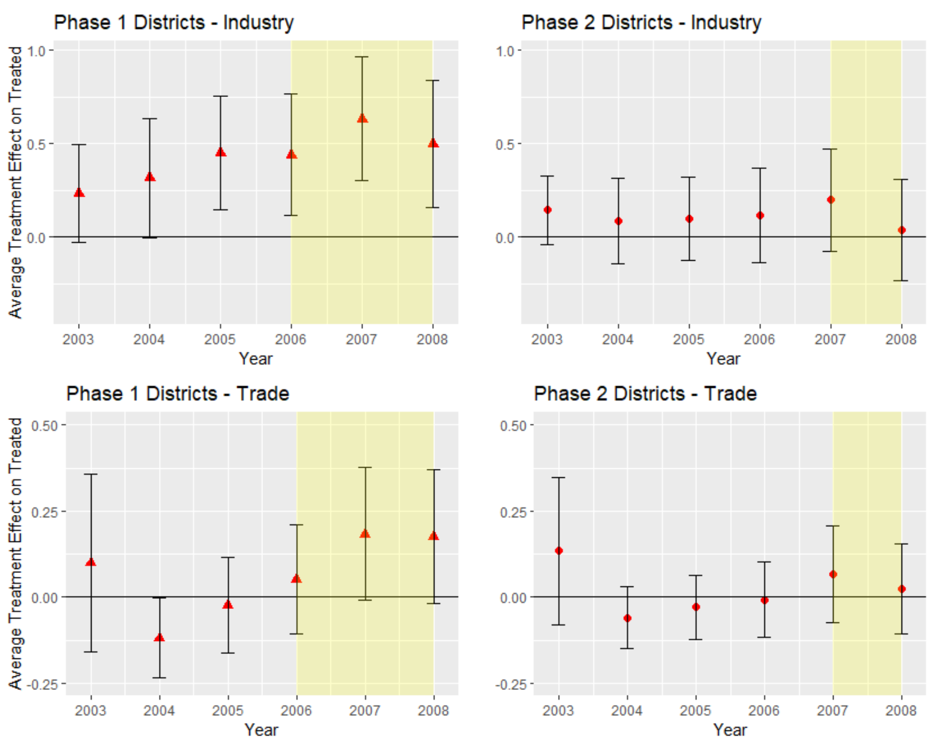


Table 6

Phase-wise Impact of NREGS Implementation on Credit and Deposits

This table reports the results of a Two-Way-Fixed-Effects staggered Difference-in-Differences estimation of the impact of implementation of the Mahatma Gandhi National Rural Employment Guarantee Scheme (NREGS) on credit. The dependent variables are log values of the credit outstanding in their respective categories and deposits outstanding. The regression specification is

$$Y_{dt} = \alpha_d + \gamma_t + \beta_1 Phase_1 \times Post_{dt} + \beta_2 Phase_2 \times Post_{dt} + \mathbf{\Omega} \sum_g Phase_g \times \times Post_{dt} \times (\mathbf{X} - \mu_{\mathbf{1}}^{\mathbf{g}}) + \mathbf{\Phi} \sum_{t=2}^{T} t \times (\mathbf{X}) + \epsilon_{dt}.$$

The covariates are demeaned by cohort group such that the βs estimate the ATTs. The sample runs from 2002 to 2008. The βs estimate the cohort-time specific average treatment effect on treated. District boundaries are defined as per 2001 census. The standard errors are clustered at the district level

	(1) Credit	(2) Deposits	(3) Agriculture	(4) Financial	(5) Professional	(6) Industry	(7) Personal	(8) Transport	(9) Trade Services	(10) Other Loans
$Phase_1 \times Post_{dt}$	0.094**	0.024	0.021	0.353*	0.084	0.199**	0.094*	-0.153	0.108**	0.028
	(0.043)	(0.019)	(0.035)	(0.192)	(0.079)	(0.089)	(0.051)	(0.113)	(0.051)	(0.070)
$Phase_2 \times Post_{dt}$	-0.003	-0.006	-0.052**	0.199	0.101*	-0.064	-0.006	-0.162*	-0.006	-0.073
	(0.035)	(0.014)	(0.026)	(0.195)	(0.061)	(0.070)	(0.039)	(0.089)	(0.036)	(0.061)
Constant	15.302***	16.138***	14.055***	9.086***	11.523***	13.168***	13.865***	10.533***	13.017***	12.544***
	(0.076)	(0.039)	(0.080)	(0.378)	(0.145)	(0.158)	(0.101)	(0.240)	(0.106)	(0.142)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demeaned Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2939	2929	2936	2849	2939	2939	2938	2939	2939	2845
R^2	0.963	0.992	0.972	0.707	0.912	0.916	0.963	0.848	0.936	0.885

Table 7

Relaxing No Anticipation Assumption

This table reports the results of a Two-Way-Fixed-Effects staggered Difference-in-Differences estimation of the impact of implementation of the Mahatma Gandhi National Rural Employment Guarantee Scheme (NREGS). The dependent variables are log values of the credit outstanding in their respective categories and deposits outstanding. The regression specification is that of equation 4, where the observations for years 2005 and 2006 are removed to alleviate concerns of violations of the no anticipation assumption. The covariates are demeaned by cohort group such that the βs estimate the ATTs. The βs estimate the cohort-time specific average treatment effect on treated. District boundaries are defined as per 2001 census. The standard errors are clustered at the district level

	(1) Credit	(2) Deposits	(3) Agriculture	(4) Financial	(5) Professional	(6) Industry	(7) Personal	(8) Transport	(9) Trade Services	(10) Other Loans
$Phase_1 \times t2007$	0.091	0.055***	-0.035	0.376	0.047	0.256**	0.064	-0.109	0.093	-0.046
	(0.060)	(0.021)	(0.042)	(0.246)	(0.095)	(0.126)	(0.066)	(0.133)	(0.059)	(0.097)
$Phase_1 \times t2008$	0.152***	0.022	0.054	-0.398	0.152	0.267**	0.226***	-0.301	0.187**	-0.066
	(0.056)	(0.035)	(0.059)	(0.348)	(0.131)	(0.134)	(0.080)	(0.200)	(0.079)	(0.117)
$Phase_2 \times t2007$	0.001	-0.019	-0.039	-0.129	0.144*	-0.066	0.026	-0.224*	0.009	-0.139*
	(0.044)	(0.023)	(0.038)	(0.254)	(0.086)	(0.097)	(0.055)	(0.132)	(0.048)	(0.081)
Constant	15.188***	16.150***	13.983***	10.323***	11.322***	13.188***	13.629***	10.454***	12.874***	12.571***
	(0.085)	(0.054)	(0.106)	(0.499)	(0.200)	(0.193)	(0.127)	(0.319)	(0.137)	(0.197)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demeaned Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2099	2091	2098	2042	2099	2099	2098	2099	2099	2040
R^2	0.962	0.992	0.972	0.718	0.914	0.913	0.962	0.845	0.933	0.890

Table 8

Alternative Estimator of Staggered Difference-in-Differences

This table reports the results of the Callaway and Sant'Anna (2021) staggered Difference-in-Differences estimation of the impact of implementation of NREGS. The dependent variables are log values of the credit outstanding in their respective categories and deposits outstanding. The sample runs from 2002 to 2008. District boundaries are defined as per 2001 census. The standard errors are clustered at the district level

	(1) Credit	(2) Deposits	(3) Agriculture	(4) Financial	(5) Professional	(6) Industry	(7) Personal	(8) Transport	(9) Trade Services	(10) Other Loans
$Phase_1$ in 2007	0.142***	-0.005	0.145***	1.32***	0.240*	0.186*	0.109**	-0.084	0.189***	0.337**
	(0.047)	(0.022)	(0.051)	(0.303)	(0.126)	(0.112)	(0.054)	(0.130)	(0.068)	(0.143)
$Phase_1$ in 2008	0.146***	-0.028	0.158**	0.195	0.240*	0.039	0.200***	-0.314*	0.168**	0.416***
	(0.048)	(0.032)	(0.069)	(0.320)	(0.143)	(0.140)	(0.077)	(0.179)	(0.067)	(0.145)
$Phase_2$ in 2007	0.045	-0.005	0.003	-0.015	0.010	-0.087	0.061	-0.218*	0.014	0.160
	(0.041)	(0.020)	(0.043)	(0.310)	(0.095)	(0.089)	(0.047)	(0.112)	(0.040)	(0.115)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2933	2929	2929	2792	2933	2933	2933	2933	2933	2788

Table 9

Simple Difference-in-Differences Estimation

This table reports the results of a *standard* Difference-in-Differences estimation of the impact of implementation of NREGS on credit. The sample is restricted to Phase 1 (treated) and Phase 3 (control) districts only. The dependent variables are log values of the credit outstanding in their respective categories. The regression specification is

$$Y_{dt} = \alpha_d + \gamma_t + \beta.Treated \times Post + \mathbf{\Omega}.Treated \times Post \times (\mathbf{X} - \mu_{\mathbf{1}}^{\mathbf{g}}) + \mathbf{\Phi} \sum_{t=2}^{T} t \times (\mathbf{X}) + \epsilon_{dt}.$$

The covariates are demeaned by cohort group such that the βs estimate the ATTs. The sample runs from 2002 to 2008. The β estimates the average treatment effect on treated (i.e. Phase 1 districts). District boundaries are defined as per 2001 census. The standard errors are clustered at the district level

	(1) Credit	(2) Deposits	(3) Agriculture	(4) Financial	(5) Professional	(6) Industry	(7) Personal	(8) Transport	(9) Trade Services	(10) Other Loans
Treated × Post	0.159***	0.010	0.102**	0.171	0.149	0.277***	0.198***	-0.166	0.179**	0.057
	(0.046)	(0.029)	(0.046)	(0.253)	(0.112)	(0.105)	(0.062)	(0.157)	(0.071)	(0.105)
Constant	15.212***	16.150***	13.965***	9.309***	11.439***	13.088***	13.779***	10.479***	12.903***	12.573***
	(0.083)	(0.049)	(0.090)	(0.414)	(0.174)	(0.177)	(0.113)	(0.278)	(0.129)	(0.165)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demeaned Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Districts	325	321	322	281	325	325	324	325	325	278
Observations	2275	2247	2254	1967	2275	2275	2268	2275	2275	1946
R^2	0.965	0.991	0.974	0.686	0.912	0.915	0.961	0.842	0.915	0.877

Table 10

Synthetic Difference-in-Differences Estimation

This table reports the results of a *synthetic* Difference-in-Differences estimation of the impact of implementation of NREGS on credit, à la (Arkhangelsky et al. (2021)). The sample is restricted to Phase 1 (treated) and Phase 3 (control) districts only. The dependent variables are log values of the credit outstanding in their respective categories. The sample runs from 2002 to 2008. District boundaries are defined as per 2001 census. The standard errors are clustered at the district level

	(1) Credit	(2) Deposits	(3) Agriculture	(4) Financial	(5) Professional	(6) Industry	(7) Personal	(8) Transport	(9) Trade Services	(10) Other Loans
Treated × Post	0.055**	-0.026*	0.02**	0.246	0.075	0.130**	0.059**	-0.113	0.028	-0.036
	(0.027)	(0.015)	(0.026)	(0.147)	(0.061)	(0.063)	(0.024)	(0.089)	(0.033)	(0.052)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Districts	325	321	322	281	325	325	324	325	325	278
Observations	2275	2247	2254	1967	2275	2275	2268	2275	2275	1946

Figure 5

Comparing Standard and Synthetic Difference-in-Differences

This figure plots the average treatment effect on the treated for Phase 1 districts estimated via the standard Difference-in-Differences and synthetic Difference-in-Differences approach.

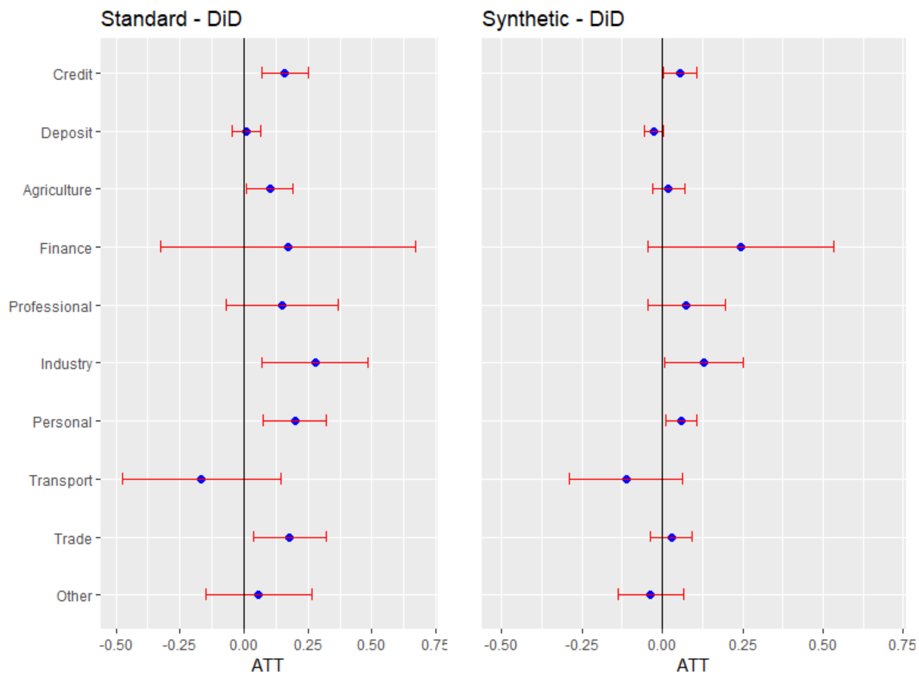


Figure 6

Project 2 is a mean-preserving spread of project 1

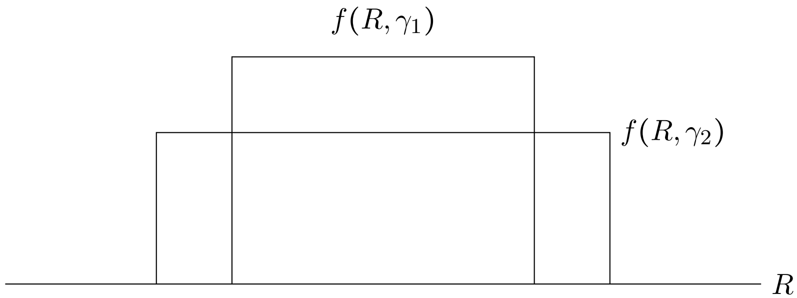


Figure 7

CDF of risk-types γ

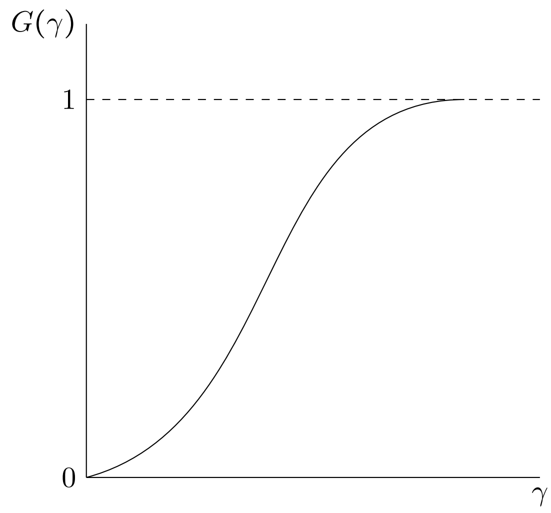


Figure 8

Bank's return is concave in R

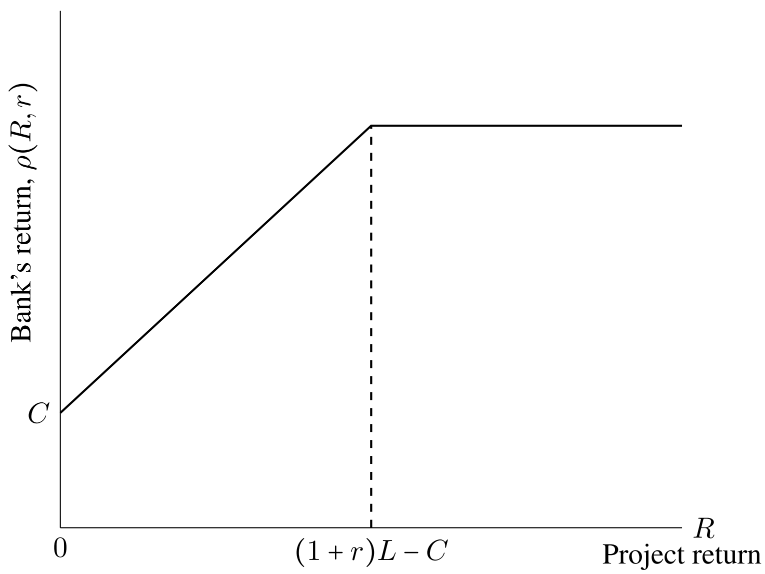


Figure 9

Interest Rates and the Marginal Borrower

Increasing the interest rate shifts up the bank's profit function, increasing profits at every risk level. However, the marginal borrower also shifts right with the increase in the interest rate. The bank's portfolio is given by the distribution of projects to the right of the threshold risk borrower, $\hat{\gamma}$. The shift in the marginal borrower from the less risky $\hat{\gamma}_1$ to the more risky $\hat{\gamma}_2$ makes the bank's portfolio more risky. The bank's expected profitability over its portfolio is given by the midpoint of the $E\rho(r)$ curve to the right of the risk threshold, $\hat{\gamma}$. A shift of the risk threshold from $\hat{\gamma}_1$ to $\hat{\gamma}_2$ reduced the bank's profitability due to a more risky portfolio.

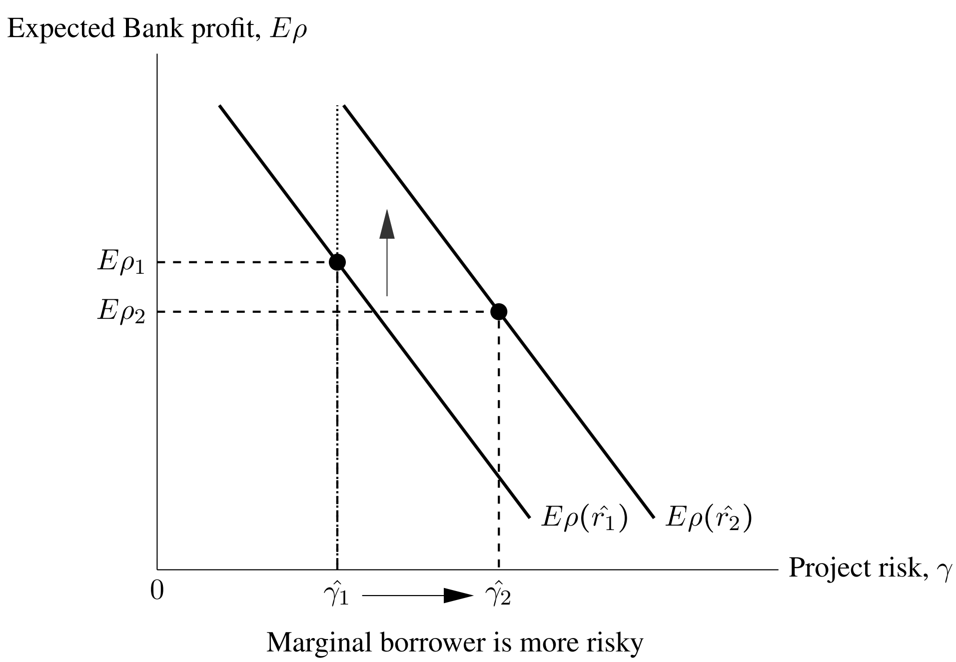


Figure 10

Bank's loan supply is increasing in bank's profitability

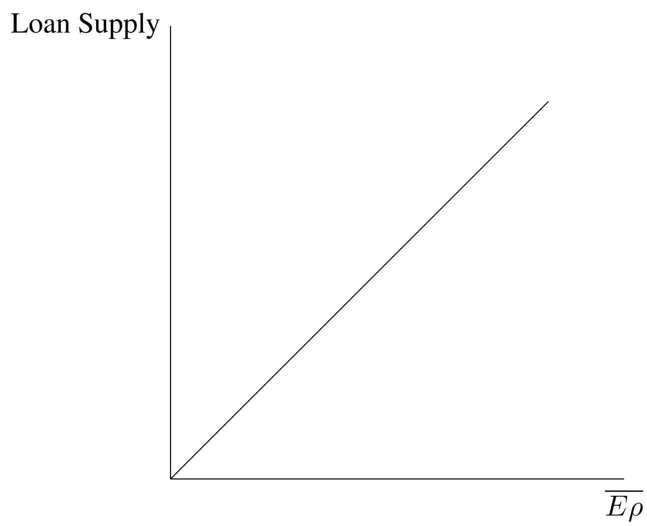


Figure 11

Bank profit and the interest rate

Bank profit is a non-monotonic function of the interest rate. Bank profit is maximized at an interior interest rate.

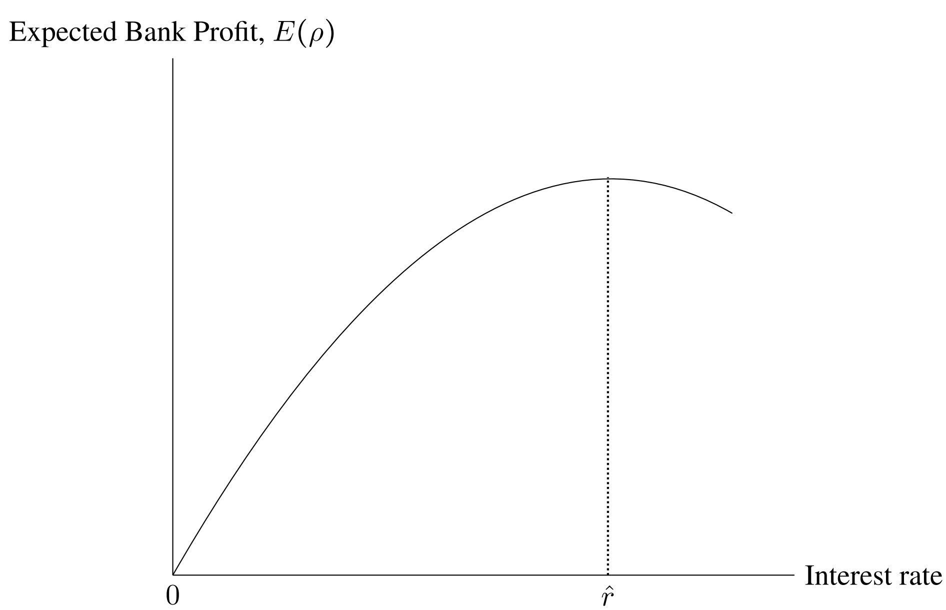
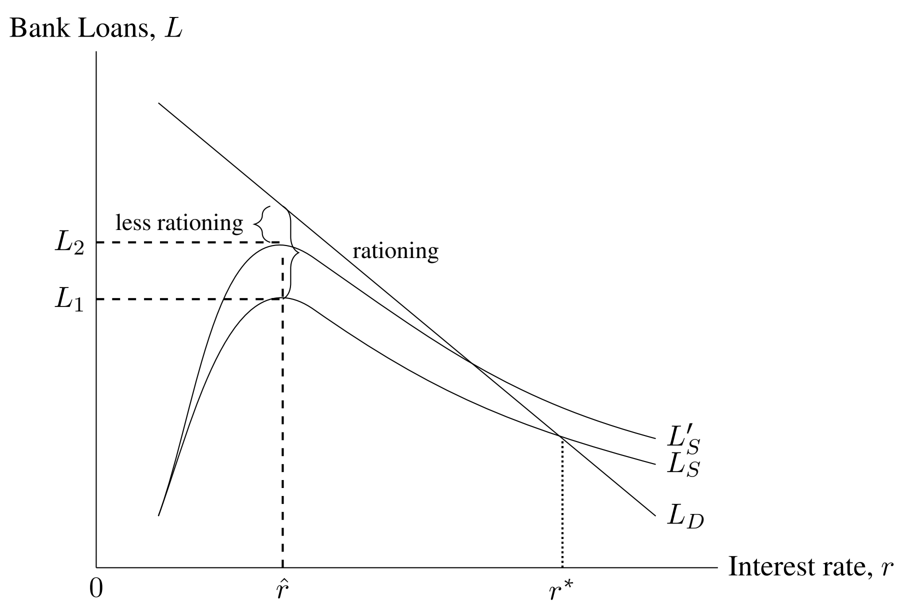


Figure 12

Credit Rationing Equilibrium

Less information asymmetries increase bank credit supply at every interest rate. The result is lower credit rationing.



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Appendix

A.1 Proofs

Lemma .1. *$E\rho(\gamma, \hat{r})$ is a decreasing function of γ, i.e., bank's profit is inversely related to the riskiness of project γ.*

Corollary .1.1. *This also means that removing the worst-risk types from the bank's portfolio will improve the bank's profitability.*

Proof. For brevity, let $\overline{E\rho(\gamma, \hat{r})}$ be denoted by $\overline{\rho}(\gamma, \hat{r})$. Then,

$$\overline{\rho}(\gamma, \hat{r}) = \frac{\int_{\hat{\gamma}}^{\overline{\gamma}} E\rho(\gamma, \hat{r}) dG(\gamma)}{G(\overline{\gamma}) - G(\hat{\gamma})}$$

is the conditional mean of bank's profits in the range of $\gamma \in [\hat{\gamma}, \overline{\gamma}]$.

The probability density function of γ is $g(\gamma)$. Then,

$$\overline{\rho}(\gamma, \hat{r}) = \frac{\int_{\hat{\gamma}}^{\overline{\gamma}} E\rho(\gamma, \hat{r}) g(\gamma) d(\gamma)}{G(\overline{\gamma}) - G(\hat{\gamma})}$$

We differentiate $\overline{\rho}(\gamma, \hat{r})$ with respect to the upper bound of γ, i.e., $\overline{\gamma}$. If the derivative is negative, then successive decreases in $\overline{\gamma}$ increase the bank's profit at every $\hat{r}$.

$$\frac{d}{d\overline{\gamma}} \overline{\rho}(\gamma, \hat{r}) = \frac{d}{d\overline{\gamma}} \left[\frac{\int_{\hat{\gamma}}^{\overline{\gamma}} E\rho(\gamma, \hat{r}) g(\gamma) d(\gamma)}{G(\overline{\gamma}) - G(\hat{\gamma})} \right]$$

Using product rule:

$$\frac{d}{d\overline{\gamma}} \overline{\rho}(\gamma, \hat{r}) = \left(\frac{1}{G(\overline{\gamma}) - G(\hat{\gamma})} \right) \frac{d}{d\overline{\gamma}} \left[\int_{\hat{\gamma}}^{\overline{\gamma}} E\rho(\gamma, \hat{r}) g(\gamma) d(\gamma) \right] - \int_{\hat{\gamma}}^{\overline{\gamma}} E\rho(\gamma, \hat{r}) g(\gamma) d(\gamma) \left[\frac{1}{(G(\overline{\gamma}) - G(\hat{\gamma}))^2} \frac{dG(\overline{\gamma})}{d\overline{\gamma}} \right]$$

$$\frac{d}{d\overline{\gamma}} \overline{\rho}(\gamma, \hat{r}) = \left(\frac{1}{G(\overline{\gamma}) - G(\hat{\gamma})} \right) \frac{d}{d\overline{\gamma}} \left[\int_{\hat{\gamma}}^{\overline{\gamma}} E\rho(\gamma, \hat{r}) g(\gamma) d(\gamma) \right] - \left[\frac{\int_{\hat{\gamma}}^{\overline{\gamma}} E\rho(\gamma, \hat{r}) g(\gamma) d(\gamma)}{(G(\overline{\gamma}) - G(\hat{\gamma}))^2} g(\overline{\gamma}) \right]$$

$$\frac{d}{d\overline{\gamma}} \overline{\rho}(\gamma, \hat{r}) = \left(\frac{1}{G(\overline{\gamma}) - G(\hat{\gamma})} \right) \frac{d}{d\overline{\gamma}} \left[\int_{\hat{\gamma}}^{\overline{\gamma}} E\rho(\gamma, \hat{r}) g(\gamma) d(\gamma) \right] - \left(\frac{g(\overline{\gamma})}{G(\overline{\gamma}) - G(\hat{\gamma})} \right) \left(\frac{\int_{\hat{\gamma}}^{\overline{\gamma}} E\rho(\gamma, \hat{r}) g(\gamma) d(\gamma)}{G(\overline{\gamma}) - G(\hat{\gamma})} \right)$$

Let the derivative in square brackets be denoted as A. Then,

$$\frac{d}{d\overline{\gamma}} \overline{\rho}(\gamma, \hat{r}) = \left(\frac{1}{G(\overline{\gamma}) - G(\hat{\gamma})} \right) A - \left(\frac{g(\overline{\gamma})}{G(\overline{\gamma}) - G(\hat{\gamma})} \right) \left(\frac{\int_{\hat{\gamma}}^{\overline{\gamma}} E\rho(\gamma, \hat{r}) g(\gamma) d(\gamma)}{G(\overline{\gamma}) - G(\hat{\gamma})} \right)$$

where

$$A = \frac{d}{d\overline{\gamma}} \left[\int_{\hat{\gamma}}^{\overline{\gamma}} E\rho(\gamma, \hat{r}) g(\gamma) d(\gamma) \right]$$

Using Leibniz rule, we get

$$A = E\rho(\overline{\gamma}, \hat{r})g(\overline{\gamma})\frac{d\overline{\gamma}}{d\overline{\gamma}} - E\rho(\hat{\gamma}, \hat{r})g(\hat{\gamma})\frac{d\hat{\gamma}}{d\overline{\gamma}} + \int_{\hat{\gamma}}^{\overline{\gamma}} \frac{\partial}{\partial\overline{\gamma}}[E\rho(\gamma, \hat{r})g(\gamma)]d\gamma$$

Simplifying using ($\frac{d\overline{\gamma}}{d\overline{\gamma}} = 1$, $\frac{d\hat{\gamma}}{d\overline{\gamma}} = 0$, $\frac{\partial}{\partial\overline{\gamma}}[p(\gamma, \hat{r})g(\gamma)] = 0$),

$$A = E\rho(\overline{\gamma}, \hat{r})g(\overline{\gamma})$$

Plugging $A = E\rho(\overline{\gamma}, \hat{r})g(\overline{\gamma})$ back into

$$\frac{d}{d\overline{\gamma}}\overline{\rho}(\gamma, \hat{r}) = \left(\frac{1}{G(\overline{\gamma}) - G(\hat{\gamma})}\right)A - \left(\frac{g(\overline{\gamma})}{G(\overline{\gamma}) - G(\hat{\gamma})}\right)\left(\frac{\int_{\hat{\gamma}}^{\overline{\gamma}} E\rho(\gamma, \hat{r})g(\gamma)d(\gamma)}{G(\overline{\gamma}) - G(\hat{\gamma})}\right)$$

we get

$$\frac{d}{d\overline{\gamma}}\overline{\rho}(\gamma, \hat{r}) = \left(\frac{E\rho(\overline{\gamma}, \hat{r})g(\overline{\gamma})}{G(\overline{\gamma}) - G(\hat{\gamma})}\right) - \left(\frac{g(\overline{\gamma})}{G(\overline{\gamma}) - G(\hat{\gamma})}\right)\left(\frac{\int_{\hat{\gamma}}^{\overline{\gamma}} E\rho(\gamma, \hat{r})g(\gamma)d(\gamma)}{G(\overline{\gamma}) - G(\hat{\gamma})}\right)$$

Simplifying, we get

$$\frac{d}{d\overline{\gamma}}\overline{\rho}(\gamma, \hat{r}) = \underbrace{\left(\frac{g(\overline{\gamma})}{G(\overline{\gamma}) - G(\hat{\gamma})}\right)}_{+}\underbrace{\left[E\rho(\overline{\gamma}, \hat{r}) - \left(\frac{\int_{\hat{\gamma}}^{\overline{\gamma}} E\rho(\gamma, \hat{r})g(\gamma)d(\gamma)}{G(\overline{\gamma}) - G(\hat{\gamma})}\right)\right]}_{-} < 0$$

The term outside the square brackets is positive. The term inside the square brackets can be interpreted as the difference between the expected payoff from the worst-risk project and the mean payoff over the entire portfolio to the bank. Since the mean payoff is at least as large as the worst-risk payoff, the square term is negative, making the derivative of bank's expected return with respect to the upper bound of risk type negative. Removing the worst-risk types from portfolio improve the bank's profitability.

□

A.2 Description of Loan Categories

Table 1

Explanation of variables

Variable	Source	Definition
Total Bank Credit	RBI BSR	The total value (in billion Rupees) of loans outstanding as of 31st March of a given year.
Deposits	RBI BSR	The total value (in billion Rupees) of loans outstanding as of 31st March of a given year.
Agriculture	RBI BSR	The value of agricultural loans outstanding as of 31st March of a given year. The loans include: (a) Direct finance to farmers, Self Help Groups (SHGs), Joint Liability Groups (JLGs), and corporate firms for growing of food crops, cash crops, plantation crops, credit for farm machinery, farm transport vehicles, soil/land development activities, irrigation, construction of pump houses and sheds, storage and marketing of agricultural produce. (b) Indirect finance to institutions that support agricultural production in rural areas. For example, finance for setting up an agriclinic or agribusiness center and loans to farm input dealers.
Transport	RBI BSR	Transport, Storage & Communications - services of land, water, and air transport of passenger and freight. Allied activities such as tour operators and travel agencies, parking lots, maintaining railway stations, piers, docks, crating, packing, sampling, etc. Storage includes warehousing and cold storage. Communications include post and courier activities, telecommunication services such as STD/ISD booths, cycber cafes, cable operators, cellular phones, maintenance (not construction) of telecom network. Examples range from credit given to Maharashtra State Road Transport Corporation to an auto-rickshaw operator or cold storage owner.

Variable	Source	Definition
Finance	RBI BSR	Includes financial intermediation services provided by (a) Financial intermediation except Insurance and Pension Funding Banks - SCBs, RRBs, Cooperative Banks, local area banks. (b) Co-operative Institutions and Land Development Banks such as Primary Agricultural Credit Societies (PACS), Farmers Service Societies (FSS) and Large sized Adivasi Multi-purpose Societies (LAMPS), Apex Co-operative Housing Finance Societies, Co-operative Marketing Societies (including loans to co-operative banks for re-lending to co-operative marketing societies) and Co-operatives of producers (for example, Aarey Milk Colony Co-operative society consisting of licencee cattle owners), Land Development Banks-SLDBs and PLDBs, Other non-agricultural credit societies. (c) Non-Banking Financial Institutions/Companies such as Development Financial Institutions which is an old system now (e.g. IFCI, SIDBI, ICICI, IDBI, NHB, EXIM Bank, DFHI, SFCs, etc.), Housing finance companies/corporations, other specialised institutions granting credit for house purchase (including those accepting deposits), Factoring/Hire Purchase Finance Companies, Chit funds, Nidhis/Mutual benefit financial companies, Financial Leasing companies (Equipment Leasing companies), Other NBFCs. (d) Other financial intermediation such as Agricultural Finance Corporations, Mutual Funds including Unit Trust of India, Shroffs and other indigenous bankers (include pawn brokers, private money lenders), (e) Insurance and Pension Funding, Except Compulsory Social Security

Variable	Source	Definition
		(f) Activities auxiliary to Financial Intermediation such as Agro-industries Corporations, Securities trading companies/broking firms, Industrial Development Boards/Corporations/ Federations including all State Development Boards and other Developmental Institutions (e.g. Tea Boards, Coffee Boards, Khadi Development Board, etc.), Merchant banking/Financial services companies, Loans for activities auxiliary to financial intermediation except insurance and pension funding (like administration of financial markets, Security dealing activities by stock/share brokers; Loans to financial advisers, mortgage advisers and brokers, bureaux de change etc.), Loans for activities auxiliary to insurance and pension funding (like insurance agents, average and loss adjusters, actuaries and salvage administrators.)
Industry	RBI BSR	Loans to industry include loans to: (a) Mining and Quarrying (b) Food Manufacturing & Processing (c) Beverage & Tobacco (d) Textiles (e) Paper, Paper Products & Printing (f) Woods and Wood Products (g) Leather & Leather Products (h) Gems and Jewellery (i) Rubber & Plastic Products (j) Chemicals & Chemical Products (k) Petroleum, Coal Products & Nuclear Fuels (l) Manufacture of Cement & Cement Products (m) Basic Metals & Metal Products (n) Engineering - machinery, equipment, appliances, weapons and ammunition, office and computing machinery, generators, transformers, TV radio, medical equipment, watches, etc. (o) Vehicles, Vehicle Parts & Transport Equipment - motor vehicles, ships, locomotives, aircraft, rickshaws, etc. (p) Other Industries - furniture, fixtures, brooms, recycling of metal waste and scrap, etc.

Variable	Source	Definition
		(r) Electricity, Gas & Water - construction of power plants (hydro-electric, thermal, coal, nuclear), collection and distribution of energy to households, industrial and commercial users, generation and distribution of solar, bio-gas, windmills, etc., collection, purification and distribution of water. (s) Construction - general as well as special buildings such as stadium and industrial plant, building installations, infrastructure construction such as erection and maintenance of power and transmission lines and power plants, telecommunication transmission lines and telecom projects, roads and ports, bridges, tunnels, pipelines, waterways, etc., water reservoirs and irrigation channels.
Personal	RBI BSR	Includes loans for: (a) Housing (b) Consumer Durables (c) Vehicles (d) Education (e) Personal Credit Cards (f) Others such as medical loans
Professional	RBI BSR	Includes loans for: (a) Professional Services (b) Tourism, Hotel & Restaurants (c) Recreation services (d) IT and Telecommunications (e) Others
Trade	RBI BSR	Includes: (a) Wholesale - including commission trade, commodity brokers, auctioneers. (b) Retail - general stores, consumer cooperative stores, fair price and ration shops, new and second hand goods (pawn shops), and include repair of personal and household goods
Other	RBI BSR	Difference between total bank credit and the sum of the different categories (per year).